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# STUDIES ON THE PHYSIOLOGY OF AWARENESS: AN OXIMETRICALLY MONITORED CONTROLLED STRESS TEST[1]

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AND ROBERT A. SCHNEIDER
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SINCE Carl Jung was originally criticized for the lack of validity of his discrete word association test for the measurement of foci of emotional conflict (1), no adequate successor has appeared. The present paper records the results of an attempt to quantify the emotional aspect of awareness by employing a variable, capillary blood-oxygen saturation, which appears to be intimately related to emotional change (2, 6). Measurements are made in a setting in which the differential stresses are entirely under the experimenter's control.

## EXPERIMENTAL PROCEDURES

### *Spectroscopic Oximetry*

The technique of this method of assessing the arterialized capillary blood-oxygen saturation has been given in detail elsewhere (3). In brief, the method involves observation, through a pocket prismatic spectroscope, of the twin absorption bands of oxyhaemoglobin of the blood in the minute vessels of the skin of the illuminated, histaminized finger nail fold, and measurement of the time taken for their disappearance after occlusion of the peripheral circulation. Before the test proper, a series of readings is taken until individual readings check to within one second. The validity of these resting oximetric observations, in terms of the differential levels obtained in a variety of psychiatric (3) and physical (12) disorders, has already been reported. A regression equation is used to interpret the raw score in Reduction Time seconds (RT secs.) in terms of percentage of blood oxygen saturation (3). Results obtained by this method correlated +.87 with alveolar oxygen tension (11) and +.92 with arterial oxygen saturation as measured by a Millikan photo-electric oximeter (3).

### *The "Controlled Stress Test" (CST)*

This consisted of 3 alternative sets of 15 stimuli, of which 7 were potentially neutral in affect and 8 potentially traumatic or stressful. Of the 15 stimuli, 10 were words culled largely from Rapaport's (10) Word Association list; of the remaining five, one was a Rorschach card, and four were picture cards selected from the TAT (9) and the MAPS Test (14). This arrangement provided both auditory and visual modes of stimulation and made it possible, by interspersing stressful stimuli among neutral ones, to compare fluctuations induced by stress with non-stress levels.

[1]The help of M. Israel, S. Cox, and G. Talland is gratefully acknowledged.

"Neutral" stimuli included such words as "mountain," "wood," "chair," and such pictures as street scenes and landscapes. "Traumatic" stimuli were such words as "blood," "hate," "nipple," Rorschach cards VI and VII, and pictures of lust, murder, and surgical operations. A rigid order of presentation of the 15 stimuli was adhered to, auditory stimuli 1, 2, 4, 6, and 8 and visual stimuli 12 and 14 being "neutral"; auditory stimuli 3, 5, 7, 9, and 10 and visual stimuli 11, 13, and 15 being "traumatic." The series were as follows:

| | *Series 1* | *Series 2* | *Series 3* |
|---|---|---|---|
| 1. | Hat | Mountain | Tree |
| 2. | Lamp | Bird | Basket |
| 3. | Love | Blood | Kiss |
| 4. | Floor | Radiator | Door |
| 5. | Fight | Hate | Fire |
| 6. | Wood | Wood | Wood |
| 7. | Nipple | Pregnancy | Mother |
| 8. | Chair | Chair | Chair |
| 9. | God | Father | Dead |
| 10. | Wife (for male subjects) Husband (for females) | Girl friend (males) Boy friend (females) | Homosexual |
| 11. | Rorschach card VI | Rorchach card VI | Rorchach card IX |
| 12. | TAT card BG 12 | MAPS card–living room | TAT card BG 12 |
| 13. | TAT card 15 | TAT card 4 (for males) TAT card 12F (females) | TAT card 18 BM (males) TAT card 3 GF (females) |
| 14. | MAPS card–street scene | TAT card 2 | TAT card 2 |
| 15. | TAT card 13 MF | TAT card 8 BM | TAT card 4 (males) TAT card 12 F (females) |

The 15 stimuli were presented separately and in the order listed. The experimenter explained to the subject that he was about to give him a series of words and pictures, and when *S* heard the first stimulus he was to tell *E* the first word that came to mind. It was impressed upon *S* that there was no right or wrong response. Before the test proper began, *E* suggested that a possible response to the word "ceiling" might be "floor" or "window," and to the word "lamp" might be "light."

Immediately *S* had responded to a word or picture the circulation was occluded, the stopwatch started and the oximetric reading taken. Cuff pressure was released as soon as the reading was completed. Series 1 of the test was used for routine testing, Series 2 and 3 being used when Ss were retested, and when reliability estimates were made.

Occasionally *S* was unable to verbalize any response to the stimuli presented. This could be due either to the patient's psychiatric condition, e.g. a catatonic stupor or senile dementia, or to an emotional conflict engendered by the stimulus. When no response was forthcoming, an oximetric reading was taken 15 seconds after presentation of the stimulus.

Administration of the test was simple, the time required ranging from 20 to 30 minutes, depending upon the co-operation of the subject and the individual oximetric status, in RT secs.

*Clinical Material*

A total of 146 subjects was examined, of whom 48 (35 males, 13 females) were healthy controls of ages ranging from 18 to 45 (mean 23.4) years. The remaining 98 subjects were patients falling into the following groups: 15 neurotics, 28 schizophrenics, 15 depressives, 13 idiopathic epileptics, 9 senile dements, 18 high-grade mental defectives. Patients included 51 males and 47 females; their ages ranged from 17 to 68, with a mean of 27.9 years.

## Results

Figure 1 gives the mean CST oximetric profile of the healthy subjects. It can be seen that responses to non-traumatic stimuli, both verbal and auditory, showed no significant difference from resting levels, whereas all stressful stimuli produced similar depressions in the blood-oxygen values. An analysis of variance was made to determine whether these findings applied also to the various psychiatric groups. The differences between the means of the traumatic (T) and non-traumatic (NT) stimuli of the CST were studied for the controls and for six patient groups. Statistically significant differences were found ($F$ ratio $= 13.466$, $p < .01$). Table I shows that the mean difference between the oximetric responses to T and NT stimuli is significant for all groups studied, save for the

TABLE I

Oximetric Responses in Healthy Controls and in 8 Groups of Patients ($t$-tests of mean differences between traumatic and non-traumatic CST stimuli)

| Group | $N$ | Oximetric means (in RT secs.) NT | T | $D$ | Confidence level |
|---|---|---|---|---|---|
| Controls | 48 | 40.925 | 33.159 | 7.767 | < .0001 |
| Neurotics | 15 | 36.495 | 26.241 | 10.254 | < .0001 |
| Epileptics | 13 | 42.011 | 22.019 | 19.992 | < .01 |
| Dements | 9 | 22.930 | 18.810 | 2.693 | < .05 |
| Aments | 18 | 27.980 | 23.940 | 4.047 | < .02 |
| Depressives | 15 | 35.695 | 27.525 | 8.170 | < .10 |
| Const. schizophrenics | 12 | 17.988 | 17.148 | 0.832 | > 10.0 |
| Paranoid schizophrenics | 16 | 23.427 | 18.266 | 5.208 | < .05 |
| Par. schiz. CST 1st half: | 16 | 27.825 | 20.184 | 7.640 | < .02 |
| Par. schiz. CST 2nd half: | 16 | 18.455 | 17.072 | 1.410 | > .10 |

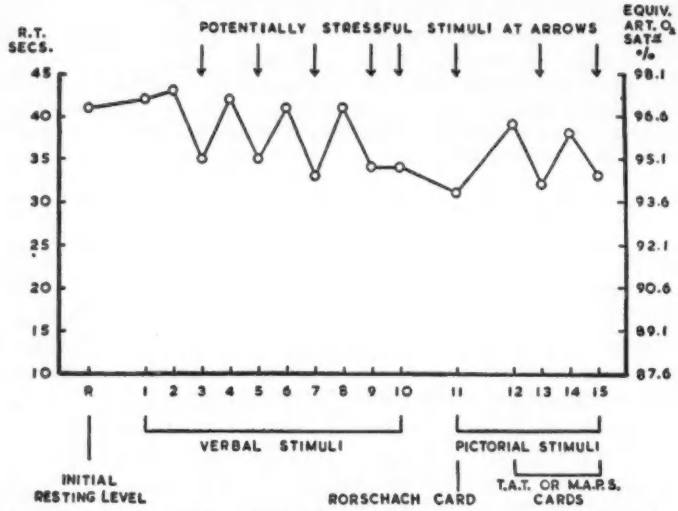


FIGURE 1. Mean CST profile of 48 healthy subjects. Note clear oximetric differentiation of non-traumatic from stressful stimuli.

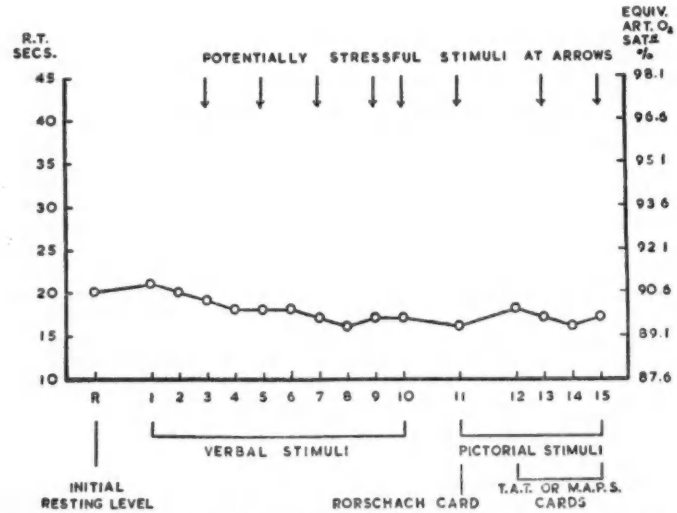


FIGURE 2. Mean CST profile of 12 constitutional schizophrenics. Mean resting level characteristically anoxaemic; no differentiation of response between stressful and non-stressful stimuli.

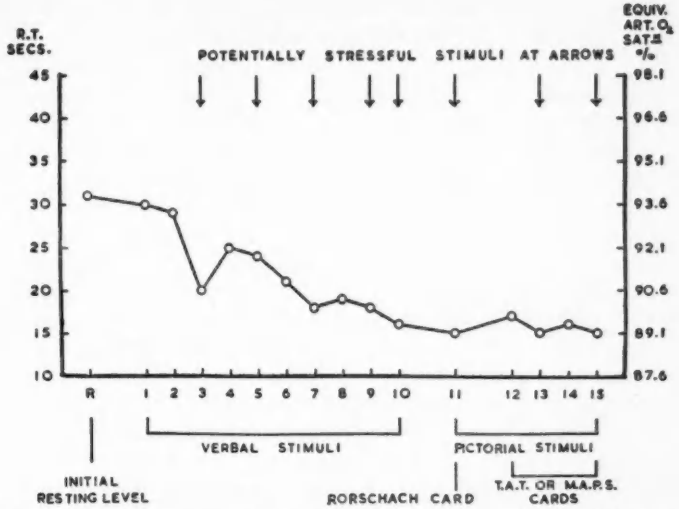


FIGURE 3. Mean CST profile of 16 paranoid schizophrenics. Note initially normal oximetric lability to stress, with summation of further stresses leading to persistent anoxaemia and lack of reactivity seen in constitutional schizophrenia (cf. Figure 2).

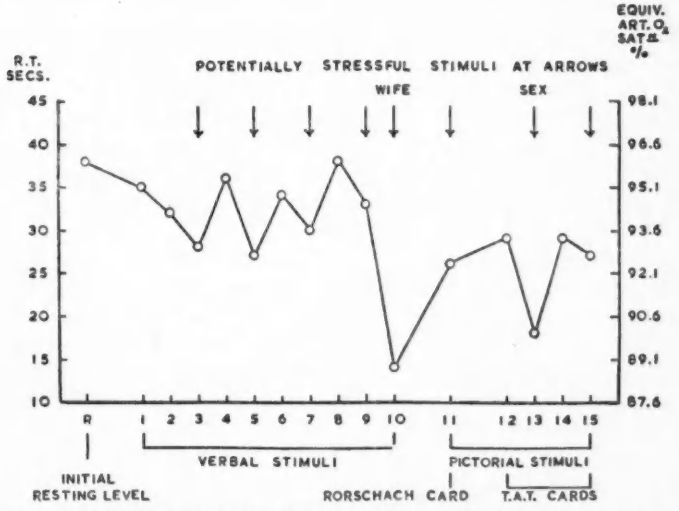


FIGURE 4. Individual CST profile of psychoneurotic male (Case 3 in text). Note excessive anoxaemic responses to stimuli with especial emotional significance for S.

cases of schizophrenia and depression. Apparently, therefore, it can be assumed that a period of relative anoxaemia accompanies the emotional situation involved in the sensory reception of visually and audibly perceived stressful stimuli for all subjects except psychotics. Among the psychotics, however, Table I shows that the responses of patients with a depressive illness are accompanied by mean differences between T and NT stimuli which, although less than those of the non-psychotic groups, are greater than those of the schizophrenics ($t = 1.98$, $p = > .1$ for depression; $t = .42$, $p = > 10.0$ for constitutional schizophrenia).

"Constitutional schizophrenia," for purposes of this investigation, was held to include those varieties of dementia praecox which comprise the syndromes of hebephrenia, catatonic schizophrenia, and dementia praecox simplex, since it has been shown (3) that these three disorders are accompanied by an identical oximetric reactivity. They are to be differentiated sharply from the "paranoid schizophrenias," which include the syndromes of paraphrenia, paranoid schizophrenia, paranoia, and some cases of oneirophrenia. The oximetric CST profiles of these two major sub-types of schizophrenia show interesting similarities as well as differences (Figures 2 and 3). The schizophrenic profile as a whole is clearly distinguished from that of the control group (Figure 1) by the anoxaemia which runs throughout it. "Paranoid" schizophrenia is differentiated from "constitutional" schizophrenia in that lack of responsivity to stressful stimuli tends to summate, as one traumatic stimulus succeeds another, to produce a final picture of decompensated reactive inertia resembling the complete CST profile of the "constitutional" group. Table I shows that if the first half of the CST profile of the "paranoid" group (stimuli 1–6) is compared with the second half of the test (stimuli 7-15) the differences between T and NT stimuli are significant for the first half ($t = 2.66$, $p = < .02$) but not for the second ($t = .74$, $p = > 10$), the latter resembling the entire profile of the "constitutional" group.

Table I also reveals additional information concerning the other psychiatric groups studied. The extreme oximetric lability of patients with idiopathic epilepsy (mean difference T and NT = 19.99 RT secs.) is in sharp contrast with the senile dement group (mean $D = 2.69$ RT secs.) and with the mental defectives (mean $D = 4.05$ RT secs.). This is in accord with clinical expectation and with the relative differences in awareness thresholds of these types of patients. It is also clinically to be expected that neurotic patients would show some accentuation of response to stimuli of a potentially disturbing and conflict-producing nature. This is borne out in the CST (mean $D$ for neurotics = 10.25 RT secs., for controls = 7.77 RT secs.).

*Reliability Study*

To assess the extent to which the observer might be influenced by knowledge of the *a priori* character of the individual stimuli employed, a reliability study was attempted in which the CST was administered serially in two of its forms under two experimental conditions. Condition O (open) corresponded in every detail to the previous routine tests. Condition C (closed) ensured that the observer was unaware of the nature of the stimuli. This was accomplished by placing a screen between observer and subject, presenting the verbal stimuli in a whisper, insisting upon a whispered response, and providing the observer with ear plugs and a tight fitting rubber bathing cap. Signals were given to the observer by a third individual when the latter received the whispered response from the subject. Alternate subjects began under conditions O and C, and CST series 1 and 2 were randomized under the two experimental situations.

The sample consisted of 9 healthy controls and 9 neurotics. Series 1 and 2 of the test each included 8 traumatic and 7 non-traumatic stimuli, 5 of which were verbal in each group. The oximetric data, in RT secs., provided measures for four means: NT and T under conditions O and C.

An analysis of variance carried out on these means showed that the difference between the mean oximetric levels on the presentation of T and NT stimuli was significant at the .01 level of confidence under either O or C condition ($F$ ratio $= 17.21$, $n_1 = 1$, $n_2 = 58$). The difference in the means between one experimental condition and the other was not significant ($F$ ratio $= .155$, $n_2 = 1$, $n_2 = 58$), and there was no significant interaction due to the combination of one set of stimuli with either experimental condition ($F$ ratio $= 1.31$, $n_1 = 1$, $n_2 = 58$). The difference of the means of the two sets of stimuli is not significantly larger or smaller under one set of conditions than under the other ($F$ ratio $=$ $110.7$, $n_1 = 1$, $n_2 = 2$).

An estimate of mis-classification was made by taking the mean oximetric levels for each S. According to hypothesis it was expected that the levels for T stimuli would lie below this mean and those for NT stimuli above it. Auditory and visual stimuli were considered both together and separately, in order to assess the relative effectiveness of each in terms of potential stress. A binomial test was employed and, since the expected proportion is one-half and the number of stimuli large, a normal distribution could be assumed. It was found that the hypothesis was confirmed ($p = .001$) for both conditions O and C, using the massed verbal and visual stimuli. Separate consideration of the two modalities of stimulation yielded similar confirmation, save that the visual stimuli under condition C were not significant.

*Comparison of Oximetric Monitoring with Reaction-Time Estimations*

To confirm the stress potentials of the traumatic stimuli employed, simultaneous reaction time estimates were made for a group of 12 controls and 12 neurotics. Measurements were made with an ordinary stopwatch to the nearest second, reaction time being considered as the interval elapsing between presentation of the stimulus and S's response (1). Mean reaction time for the total (167) number of NT stimuli was 2.64 seconds and for the total (191) of T stimuli was 3.00 seconds, a statistically significant difference ($t = 3.42$, $p < .001$). Concurrent measurements by spectroscopic oximetry yielded mean differences from resting level readings of −1.383 and −9.130 RT secs. for the two sets of stimuli respectively. These also are statistically significant, and more so than the reaction time differences ($t = 11.17$, $p < .001$). As a considerable proportion of the differences reflected the very long reaction times of four of the patients, it was decided to exclude these and reapply the tests of significance to the remaining 20 S's. It was found that reaction time no longer differentiated between NT and T stimuli, the difference between the mean reaction times (2.06 and 2.34 secs. respectively) being statistically insignificant ($t = .79$, $p > .4$). Spectroscopic oximetry on the same group of 20 S's, however, still discriminated between the two sets of stimuli ($t = 16.7$, $p < .001$).

An estimate of the degree of interaction of the factors involved in the two methods of measurement was provided by the calculation of correlation coefficients on the same S's. For the total of 24 S's, $r$ was insignificant both for T (.10) and NT stimuli (.09). Exclusion of the four patients showing abnormally long reaction times improved the correlation co-efficients, that for NT stimuli becoming −.27 while that for T stimuli reached statistical significance at $r = .58$.

## Oximetric Evaluation of Foci of Emotional Conflict

Apart from the use of the CST to confirm psychiatric diagnosis, there exist a number of other possible applications. One of the more important of these lies in the evaluation of specific foci of emotional conflict. Provocative stimuli which appear relevant to the patient's emotional status may be inserted into one of the three series or substituted for it. An example of the increased tendency to an anoxaemic response to specially chosen stimuli is given in Case 1 below.

*Case 1.* Mr. R. W., aged 33, a delinquent psychopath, was referred to the laboratory for testing. The form of delinquency to which he appeared addicted was that of the larceny of bicycles; he was not apparently interested in stealing anything else but had, at the time of examination, stolen five of these vehicles over a period of three years. He was given Series 1 of the CST with the words "father" and "bicycle" added

to the list of auditory stimuli, and a MAPS picture (a street with various vehicles obvious in it) inserted among the visual stimuli. The results of the test in profile form confirmed the history. The word "nipple" evoked the response "tire" (oximetric reading = 20 RT secs., NT stimulus and resting level = 48 RT secs.). The only other stimuli inducing such a marked anoxaemic response were the words "wife" and "bicycle," Rorschach card VII, and the street scene referred to above. Other potentially stressful stimuli, unrelated to the specific points of emotional conflict in this patient, induced anoxaemic responses sensibly lower than those accompanying non-stressful stimuli, but not reaching the very low levels evoked by the specific stimuli.

*Case 2.* Mr. J. K., aged 30, a patient suffering from a chronic anxiety state with depression, was referred by a psychologist colleague who had administered the MMPI to him. The psychologist had made a detailed analysis of the patient's responses to the MMPI statements. To five of these she felt that his responses had been deliberately falsified, since they were out of keeping both with his history and with his responses to other tests. The patient was given a modified CST which consisted of the five MMPI cards in question, interspersed among five other cards, the responses to which had not been so queried. Four out of the five cards in question were accompanied by highly significant depression in the oximetric levels, whereas the remaining card together with the five "neutral" cards produced only slight changes.

Quite often, however, no change in the routine CST stimuli is required. Figure 4 illustrates the response profile of a neurotic whose foci of emotional conflict were quite specific.

*Case 3.* Mr. T. W., aged 29, came into hospital complaining of feeling of tension and anxiety in the evenings when he returned home from his work. Throughout the day he was a fit and happy man; only when his work was finished did his symptoms begin to trouble him. Series 1 of the CST was given him shortly after his hospitalization and revealed clearly (Figure 4) the sexual nature of his conflict and the ambivalence of his feelings towards his wife.

### Changes in CST Profile with Variaton in Psychiatric State

With such marked differences in the CST profiles of different categories of psychiatric disorder, changes might be anticipated when the psychiatric state of an individual patient is examined in longitudinal section. Many such instances have been studied, both those in which clinical changes were due to unknown factors and those in which they were evoked by various modes of psychiatric treatment. These applications are illustrated in Tables II and III, respectively.

### Discussion

Emotional ("visceral") awareness may be defined as that aspect of consciousness wherein the subject is aware of a certain quality of feeling, together with the physiological overtones accompanying it. Such awareness is clearly distinguishable from other parameters of consciousness, such as cognitive, attentive, discriminatory, and conative awareness, although a degree of interdependence almost certainly exists

TABLE II

CST AND LONGITUDINAL ILLNESS PATTERN

(Miss M.L., age 22; schizoaffective disorder showing lability. Serial CST at monthly intervals, showing relationship of oximetric profile to changes in mental state.)

| Oximetric means (RT secs.) | | | Clinical status and psychological testing |
|---|---|---|---|
| Resting | T | NT | |
| 25 | 22 | 22 | Headache; ideas of reference; irritable depressive anxiety. Rorschach, Zaslow, TAT, MAPS, Goldstein: "no disturbance of conceptual thinking." |
| 27 | 17 | 20 | Depressed. Spends much time weeping. Nightmares and hypnogogic hallucinations. |
| 41 | 29 | 37 | Considerably better. I.Q. (R.P.M.): 126; I.Q. (M.H.V.): 121. |
| 27 | 13 | 18 | Condition worse. Overt lesbian activity. Depression, impulsiveness, anorexia. Paranoid thought disorder. MAPS: "marked depressive anxiety." Behn Rorschach: "autistic logic; bizarre responses; ?schizophrenia." |
| 40 | 34 | 37 | Sleeping and feeling better. Expressing feelings more openly. Clinically much improved. |
| 28 | 23 | 27 | Headaches returning. Feelings of levitation; exhaustion; homosexuality; anxiety; agitation; self reproach. Realizes she is worsening. |
| 18 | 9 | 12 | Impulsive homicidal behaviour; screaming bouts; suicidal attempts; autism; autochthonous delusions; paranoid ideation. Hallucinating freely. *Flexibilitas cerea.* |

among all of them. By analogy with the estimation of the level of cognitive awareness by means of intelligence tests, the measurement, however crude, of a variable which consistently changes, both in temporal identity and in similar order and extent, with emotional awareness, amounts in fact to the measurement of this parameter of consciousness. One such variable would appear to be the capillary blood-oxygen saturation, as estimated oximetrically.

The evocation of emotion by word or picture symbol stimuli is a commonplace in many psychological and psychotherapeutic procedures; that an aspect of this emotion may be quantified within a standardized situation is suggested by our results here. But similar quantification is possible as well in an unstructured situation in which conversation is employed (5), as when the evocative stimuli are represented, not by pictures, but by undifferentiated colour (13); by a series of pleasant or unpleasant smells (7); by the suggestion of a "dream" during hypnosis (4); by agents producing pain of various sorts (5); by drugs inducing psychotic states (15); by simple tactile, visual or auditory stimuli following each other at certain predetermined frequencies (8);

TABLE III

CST AND EFFECTS OF PSYCHIATRIC TREATMENT

(Changing physiological status of 9 hospital patients, monitored by CST before and after treatment.)

| Psychiatric diagnosis | Spectroscopic oximetry (RT secs.) | | | | | | Clinical results of treatment |
|---|---|---|---|---|---|---|---|
| | Pre-treatment | | | Post-treatment | | | |
| | Resting | T | NT | Resting | T | NT | |
| Phobic anxiety state | 32 | 21 | 34 | 40 | 27 | 35 | Symptom-free |
| Depression, with bronchial asthma | 27 | 19 | 22 | 46 | 32 | 36 | Free of depression and asthma |
| Depression with bronchial asthma | 32 | 15 | 29 | 41 | 27 | 38 | Relieved; free from asthma |
| Paranoid schizophrenia | 22 | 13 | 15 | 40 | 29 | 38 | Partial remission |
| Hebephrenia | 21 | 21 | 21 | 26 | 21 | 23 | Essentially unchanged |
| Hebephrenia | 17 | 18 | 18 | 31 | 20 | 31 | Improved |
| Acute schizophrenic reaction | 12 | 9 | 12 | 44 | 34 | 45 | Complete remission |
| Catatonic stupor | 22 | 22 | 22 | 89 | 42 | 85 | Improved |

etc. Common to each of these emotions, however produced, is an oximetric change, the direction and extent of which seem to be consequent both upon the intensity of the affective charge and also upon its hedonic quality, for pleasure is euoxaemic and its absence connotes anoxaemia.

In the present report we have seen that it is possible to control and formalize auditory and visual stimuli in respect of their ability to uncover or generate emotions, and we have tried to show *inter alia* that similar stimuli differ in their measurable emotional sequelae, depending upon the psychiatric status of the subject.

## SUMMARY

(1) The CST is a visuo-auditory test monitored by spectroscopic oximetry. It was administered to groups of healthy and psychiatrically disturbed subjects.

(2) It was found that the selected non-stressful stimuli used did not disturb the resting capillary blood-oxygen saturation, but significant anoxaemia accompanied the presentation of potentially stressful stimuli in all subjects save the psychotics.

(3) Characteristic profiles were obtained for the control group and for many of the psychiatric disorders studied. Reactivity was maximal in the epileptics, and fell off progressively in the neurotics, controls, aments, dements, and depressives. Constitutional schizophrenics showed no reactivity; paranoid schizophrenics showed an initial response and a final absence of reactivity with summation of stress.

(4) Some applications of the CST are described.

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# THE INTERRELATIONS AMONG SOME BEHAVIOURAL MEASURES OF ANXIETY[1]

LEON J. KAMIN, DALBIR BINDRA, JAMES W. CLARK, AND HELENE WAKSBERG
*McGill University*

THE CONCEPT of anxiety has assumed an imposing explanatory burden in contemporary psychology. To be over-shy or over-bold, to be functionally deaf or to hear menacing voices, to suffer from constipation or from diarrhoea—all these behaviours have been interpreted as consequences of anxiety. The anxiety concept has tended to become the keystone of clinical psychological theory.

Attempts at accurate measurement of individual differences in anxiety have not, unfortunately, kept pace with the theoretical ferment. A fundamental difficulty appears to be that the concept does not refer to a single unitary trait, but rather to a number of independent dimensions of behaviour. The common-sense term "anxiety" is connotatively rich, but the numerous "pure" variables (dimensions) included within the term are yet to be isolated.

The present study is concerned with delineating some of the dimensions which appear to be represented in anxiety. Several tests of anxiety were studied in relation to performance in a stressful, "anxiety-eliciting" situation. We believed that the tests might measure a number of non-related variables, each of which would account for some part of the variability in behaviour under stress. This study is proposed as a first step toward a dimensional analysis of the vague concept of "anxiety."

## METHOD

The subjects (Ss) were 58 adult males who had volunteered for army service. The Ss were restricted to men who had completed at least 7 years of school; they ranged in age from 17 to 34 years.

The Ss were required to perform four tasks (tests), each of which was assumed to bear some relation to the common-sense concept of anxiety. The pivotal task was a conditioned discrimination problem. We were concerned with the relations of the other tasks to conditioned discrimination, and with the intercorrelations among tasks.

### *Conditioned Discrimination*

The apparatus and procedure employed in the discrimination problem have been described in full elsewhere (4). The experimenter (*E*) controlled two similar but

[1]Research done under the auspices of DRB Grant 430, PCC No. D77-94-35-46.

discriminable buzzers. The subject was told that the two buzzers would be sounded in random sequence, and that one buzzer would always be followed by electric shock to his finger, while the other buzzer would not. He was instructed to learn, during 12 training trials, which buzzer was followed by shock. The interval between buzzer-onset and shock was 500 msec.

The subject's attention was then called to a reaction key, and 36 testing trials (18 with each buzzer) were given. He was told to press the key quickly when he heard the "dangerous" (shock) buzzer, and informed that if he pressed quickly enough (within 500 msec.) he would avoid shock. He was also instructed never to press the key in response to the "safe" (no shock) buzzer. We were primarily interested in the number of *false reactions* (key-pressing responses to the "safe" buzzer) made by S. We assumed that the more "anxious" or "jumpy" the S, the more likely he would be to make false reactions (3, 5). Performance on other tests of anxiety should therefore be related to performance on the discrimination task.

The latencies of Ss' responses in the discrimination task, as well as the number of false reactions, were recorded. This was done on the assumption that—since there was no evidence of a learning effect within the test trials—response latency would serve as another index of S's motivation. The more motivated ("anxious to avoid shock") the S, the more rapidly he should respond (4).

*Pain Tolerance*

The apparatus and procedure for this test have been described fully by Clark (1). The apparatus consisted of a two-chambered, asbestos-walled box. The upper chamber contained a 250-watt infra-red bulb. The two chambers were separated by a sheet of asbestos, in the centre of which was a circular hole 17 mm. in diameter. The subject inserted his forearm, liberally coated with lampblack, into the lower chamber; when the bulb was lit, the heat so generated was focused through the hole to a spot on his arm. He was instructed to inform *E* when he first felt pain on the arm, and again when the pain became so intense that he wished *E* to turn off the bulb. To safeguard against burns, *E* turned off the bulb 45 seconds after its onset on the few occasions when S had not already asked him to do so. Three trials were conducted, each with a new focus point on the arm.

The index of pain tolerance employed was the *difference* (in seconds) between the time when S first reported pain and the time when he requested the bulb to be turned off. The pain tolerance index is thus a measure of the length of time S is willing to tolerate a continuously increasing pain. The index is partly independent of the absolute magnitude of the pain. We assumed that "anxious" Ss would tolerate little pain.

*Time Estimation*

The procedure for this test was similar to that reported by Falk and Bindra (2). The "production method" of time estimation was employed. S was requested to produce a time interval of 15 seconds by first depressing and then releasing a key. The median production for 5 trials was computed. The scores of different Ss were arranged in order of absolute magnitude, without reference to accuracy. As reported earlier (2), almost all Ss produce intervals shorter than the required 15 seconds. We supposed (cf. 2) that the more "anxious" the S, the shorter would be the interval he produced.

*Taylor Scale*

The revised form of Taylor's scale of "manifest anxiety" (6) was given to the Ss. The scored test items only were given; filler items were omitted. The Taylor

scale, based upon items selected from the Minnesota Multiphasic Personality Inventory, gives a more "clinical" measure of anxiety.

The order in which the various tests were administered was: time estimation, conditioned discrimination, Taylor scale, and pain tolerance. The number of Ss varied considerably from test to test, owing to administrative conditions.

## Results

The correlations with which we were concerned are presented in Table I. They are rank-order correlations (rhos), since some of the distributions were skewed. The *N* upon which each coefficient is based is indicated in parentheses below it. The signs of the coefficients were determined by setting each distribution of test scores in terms of increasing magnitude. Thus, high scores on false reactions and the Taylor scale—but low scores on latency, pain tolerance, and time production—indicate "anxiety."

TABLE I

Table of Intercorrelations (Rhos)

(Numbers in parentheses indicate the *N* on which rho is based.)

| | Number of false reactions | Median latency of true reactions | Taylor score | Median pain tolerance | Median time production |
|---|---|---|---|---|---|
| Number of false reactions | | −.53** (30) | .40* (25) | −.36* (30) | −.29 (25) |
| Median latency of true reactions | | | −.09 (25) | .36* (30) | .25 (25) |
| Taylor score | | | | −.10 (50) | .10 (40) |
| Median pain tolerance | | | | | .17 (50) |

*Significantly different from zero at the 5% level.

**Significantly different from zero at the 1% level.

## Discussion

The basic findings of the study are the significant relations between (*a*) false reactions and Taylor score (.40), and (*b*) false reactions and pain tolerance (−.36). These relations mean that the more "manifest

anxiety" scored by S, and the less his tolerance of pain, the more false reactions he makes. The relation between false reactions and time estimation, though in the expected direction, is not significant. The time estimation test, therefore, is not further considered.

The central fact for us is that Taylor score and pain tolerance are two distinct, unrelated variables. Although each of these measures is related to false reactions, the relation between them is exceedingly small (−.10). The fact that pain tolerance relates significantly to response latency in the discrimination task, whereas Taylor score does not, also suggests a fundamental difference between these two "tests of anxiety."[2] We must therefore seek separate interpretations of our two main findings. Whatever factor accounts for the relation between false reactions and pain tolerance cannot at the same time account for that between false reactions and Taylor score.

There are, happily, at least two obvious ways in which false reactions may occur. We will deal first with only one. Those Ss who tend to respond very quickly to the buzzers are likely to respond before being certain which buzzer has sounded, and before discrimination can occur. This would result in many false reactions. There is, it should be noted, a significant relation (−.53) between false reactions and response latency; quick responding is associated with many false reactions.

The false reaction–pain tolerance relation can be interpreted by postulating an *avoidance tendency* to be involved in both tests. We assume that Ss with high avoidance tendency are more likely to make avoidance responses, and to make them quickly, than are Ss low in avoidance tendency. Thus, Ss high in avoidance tendency are likely both to tell *E* to stop the heat more quickly in the pain tolerance test, and to press the key more quickly in the discrimination task. This would account for the false reaction–pain tolerance relation. At the same time, this reasoning implies a relation between pain tolerance and response latency, which has been shown to exist (Table I).

We turn next to the relation between Taylor score and false reactions. The factor (avoidance tendency) common to pain tolerance and discrimination cannot account for the Taylor score–false reaction relation, since there is no relation between Taylor score and pain tolerance. We conjecture that the Taylor score is a rough index of the degree of *general upset or arousal* that S is likely to show under stress. We assume further that Ss who are highly upset find it difficult to concentrate and to discriminate between two similar stimuli. This impairment of concentration

[2]This basic difference between the Taylor scale and pain tolerance measures of "anxiety" remains if we consider the latency of *false* reactions only. Pain tolerance is related .48 (1% level) to false reaction latency; the corresponding rho for the Taylor scale is a nonsignificant −.17.

constitutes a *second* source of false reactions, and accounts for the Taylor score–false reaction relation. There is, however, no need to assume a Taylor score–response latency relation, and none was found.

We are, in brief, inclined to interpret the obtained intercorrelations in terms of two factors or variables: avoidance tendency and general upset. These two factors, each accounting for a part of the variability in performance under stress, are not identical and appear to be unrelated. Put loosely, avoidance tendency facilitates *S*'s performance of a specific and effective avoidance response; general upset serves only to perturb the *S*, without necessarily facilitating an avoidance response. Pain tolerance could be considered a measure of avoidance tendency, and the Taylor score an index of upset or arousal under stress. We do not claim that one or the other of these tests has a greater right to be called a test of anxiety. We do not believe anxiety to be a unitary trait. We think of these tests, rather, as representing two separate behavioural dimensions relevant to the description of individual differences in what is vaguely called "anxiety." Progressive experimental refinement of these dimensions, and delineation of other such dimensions, should enable us ultimately to replace the vague term by explicit and precise behavioural variables.

## Summary

The intercorrelations between three tests of "anxiety" and performance on a stressful conditioned discrimination task were studied. Taylor's scale of manifest anxiety and a test of pain tolerance were both significantly related to performance on the discrimination task, but a test of time estimation was not. There was no significant relation between the Taylor scale and pain tolerance. The intercorrelations were interpreted in terms of two independent factors: avoidance tendency and general upset or arousal. These factors can be regarded as representing two of the many dimensions that are involved in "anxiety."

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# A NOTE ON FERGUSON'S LEARNING-ABILITY MATRIX

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THERE is an interesting parallel between the methodological proposal Professor Ferguson is making in his recent paper (4) and that of John Dewey in the 1920's. The latter at that time was effecting his vastly influential transition from experimental method in the physical sciences to the experimental method as the prototype of all thinking, and thence to learning as the practice of the reflective process. "Experimental procedure" he was saying "is one that installs doing at the heart of knowing" (2). There is a productive analogy between Dewey's methodological stroke and Ferguson's. The former, in equating reflective thinking with problem solving and these with learning, made it possible to translate experimentalism into a teaching method. The latter, in installing learning at the heart of ability, may well make it possible to translate heretofore largely untested notions concerning human abilities into experimental terms, and thus ultimately make possible a more carefully ramified theory. Apart from the limitations which logic and recent social knowledge have placed on Dewey's theory, the methodological stroke remains influential. This is the initial point of analogy with Ferguson's proposal.

Dewey's methodological innovation came largely before the assemblage of anthropological and sociological data into synoptic accounts of human development, i.e., before the latter had delivered us from cultural parochialism and myopia in the matter of human learning and development. It is not altogether surprising, therefore, that he did not escape from a methodological error inherent for generations in philosophy; namely, an overconfidence in the provision which logical modes of the mind can make for an adequate theory of knowledge. Ferguson, on the other hand, profiting by recent experience in social dynamics, is by implication anxious to evade the equally rigid unidimensional models employed in some current socio-cultural theory. It is extraordinarily difficult to be sure-footed in these slippery places. One must put sufficient stress on the existential interdependence of learning and development to supply a corrective for excessive isolation and abstraction of factors. But one must at the same time provide for the dynamic elements that enter into learning. It is on this latter requirement that Dewey is ultimately



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unsatisfactory. Ferguson's position here requires substantial clarification before its full implication can be seen. There seems to be at the moment a need, in psychology at least, for the study of relationships and the interaction of variables in human learning and abilities, rather than of isolated factors. In this connection, particularly, Ferguson's proposal will be welcomed by many psychologists.

The purpose of this initial response to Ferguson's paper is to emphasize the usefulness of the dynamic model in such theory building and to raise for clarification one aspect of the operational definition of ability.

There is an instructive point of contact, it would seem, between the paper under discussion and Ferguson's recent note on Berkeley (5). Although the learning-ability paper was published after the Berkeley note, the latter contains a statement concerning the theoretical model which can profitably be related to the limits of learning theory. In the note on Berkeley, Ferguson is more specifically concerned with the "metaphysical climate" of contemporary psychology and the need to re-emphasize "mind" and learning processes as central to a theory of human development. In the course of the discussion the concept of model is defined as "a tool for deducing certain results amenable to observational or experimental tests" (5). Generalized theory is viewed under the radix of the dynamic model, and is distinguishable from, and generally opposed to, that type of phenomenological postulate which aims at isomorphic relationship with reality. The distinction is pointed up in a Berkeleyan context. One might want to question Ferguson's interpretation of Berkeley, but that would be irrelevant here; what is important to keep in mind is that when a leap is made from what can be conceived to the character of fact (which, it might be argued, was Berkeley's error), or vice versa (which is the substance of Ferguson's objection to the "current confusion" of postulational and phenomenological constructs), theory is fraught with methodological hazard.

In any case, the paper on learning and ability may be seen to be a specialized confirmation of the notion of the theoretical model. Any generalized theory, such as the limits of learning theory, may be regarded as a "tool," a device within which relevant results are initiated, sponsored, "deduced," and made available for "observational or experimental test." It is an exploratory device, the "results" of which depend for veridity on experiment and observation. Hence there is a signal value placed on experimental feasibility. Accordingly, full analytic discussion of the proposals must wait until data are available. In the meantime, however, clarification of assumptions and definitions is in order.

A fairly lean group of initial assumptions may be seen to comprise the structure of the learning-ability model. Among these may be initially

identified the following:

(1) Cultural factors prescribe what shall be learned and at what age.

(2) Learning is associated with an initial phase of the organism's actualization. In the organism's tendency to respond subsequently in terms of previous learning arises the phenomenon of transfer.

(3) There emerges in the individual organism a patterning of such modes of actualization, which continues after early learning has yielded to the transfer phenomenon, which leads along functional continua to abilities.

(4) Each ability is identified by a skill component (or components in reciprocity?) which has reached a crude performance limit by repetition.

(5) Abilities emerge along continua characterized by the differential transfer effects of prior learning, age, socio-cultural requirements.

Operating ubiquitously throughout these, there appears to be a general assumption that there is very little that is antecedently real in the matter of abilities. In the developmental emergence of operational intelligence from sensory-motor assimilation, however, *in virtue of the continuity*, successive structures presumably are affected. Certain facilitations (and the reverse) result.

Implicit is the view that the very young child is critically plastic and receptive to changes occurring in his environment. It is with respect to the problem of early learning generally, however, that Ferguson's theory seems uncertain. Survival value is given as the criterion for the selection by the culture of an activity to be prescribed for repetition by the child. A second assumption made is that this learning does not, in most instances, reach the crude limit that constitutes it an ability before adolescence. Yet there is now general recognition that, with respect to perceptual tasks at least, the limits of learning may occur at a very early, perhaps even at a *presocial*, age (**1, 6**). It may well be that, prior to a stage when social prescriptions could reasonably be said to have a marked effect even upon the most docile child, the child may have a fairly broad repertoire of abilities, not all of which are simply reflexes, and not all of which depend for their origin upon their survival value. Nevertheless, these may be of prime importance in the phases of the organism's actualization.

Further, an ability under this view is not quite the locus of learning, but it clearly lies along the locus and represents for practical purposes a *stasis* or terminus. In these terms, it is not clear what the structural status of an ability would be. Is its underlying mechanism different from adjacent points on the learning locus? In an operational situation,

is "ability" the name we give to cooperation effected among several skill components (functioning at crude limits) and brought into relationship for a specified task? Or is an ability, more simply, the point along a functional continuum at which repetition fails to effect further performance increment? If the latter, in what way is "ability" anything other than a reification of a functional concept? Although Ferguson states on page 96: "'ability' is defined operationally by the performance of an individual in a specified situation," it becomes clear, by page 99, that the term has acquired "surplus meaning." It has become an "hypothetical construct" in Meehl and MacCorquodale's usage (8). In fact, ability now must have two aspects: certain symptomatic skill components, plus certain physiological correlates. If this is the case, then the term "ability" under the operational definition is not adequate.

The situation seems now to be as follows: Certain skills which intuitively may be said to bear an isomorphic relation to ability can be operationally defined. It is these skills which reach a crude limit of learning. In addition, however, there is another aspect of ability which is not at present capable of operational definition. This is the structurally denotative aspect. What would seem to be required is a further examination of the nature of the relation that obtains between "skill" and "ability," to ensure that "skill" is not the primitive term here and "ability" a reification.[1]

In the course of his discussion Ferguson makes the useful observation that the concept "intelligence" is no longer a scientifically fruitful one. He substitutes for it a number of abilities which he considers to be relatively independent and unidimensional. These are the elementary building blocks, the units of his analysis. Abilities are overlearned acquisitions, operationally defined by the performance of an individual in a specified situation. This seems clear enough. But he goes a step farther and states: "an individual may possess the necessary ability to perform a task adequately, but may lack the ability to learn to perform the task under particular learning conditions" (p. 110). Apparently, therefore, the possession of a particular ability by the individual does not guarantee its expected transfer automatically to a new situation. In the theoretical framework, it is difficult to visualize how this could be the case. If abilities, arranged in a hierarchy of practice, are not run off automatically when required, then some unspecified factor is introduced into the system. This factor needs to be specified.

There is a temptation in all this to see certain similarities between

[1]Clarification of classes is a *sine qua non* in this matter if "ability" is to function analytically in the other terms of the matrix. Otherwise ability might be too easily dismissed altogether through any one of the ruses of the logical positivist.

"ability," as Ferguson defines it, and underlying personality mechanisms.[2] One is tempted to translate the term up the scale of learning, to apply it at higher levels of thinking and relate to it the disproportionate effort required to induce ability at a late stage of learning. This would open one aspect of the phenomenon of set—the interesting problem, for example, of introducing a mind trained in literature to statistics.

There is another aspect of the definition of ability which may deserve clarification. This concerns the logical status of the term "potentiality" in this context. It becomes much more than a linguistic boggle when the theory is envisaged as having instrumental educative effects. Ferguson remarks,

> In our culture children are exposed to an environment that demands rapid learning of many things. They proceed as rapidly through the school system as their abilities at any stage will allow. It is probable that many children at any particular age are functioning fairly close to the limit of their potentiality with regard to certain classes of activity [4, p. 99].

A little later comes the remark: "Differences in ability are the result of the complex interaction of the biological propensities of the organism, prior learning and the age at which prior learning occurs" (4, p. 110). Of the two statements regarding potentiality ("propensities" seems to represent an identical value), the latter communicates more readily. Presumably there are in the learning-ability matrix some rough and ready limits of the motor-sign type which distinguish one organism from another—some rough anatomical differences. In the case of the first quotation, however, it is hard to see within the furnishings of the model how potentiality is to be characterized. Indeed, in all theories of intelligence which employ the dynamic properties of a brain model (including Hebb's *Intelligence A* (6)), it is difficult to see just how potentiality can be characterized other than as a necessary mental construct. Potentiality threatens all the while to fall apart into a powder of moments and positions along a path which in theory must be a continuum.

Is the potentiality of the child to learn analogous to the potentiality of a balloon to expand or a rubber ball to bounce? If so, this locates the "potential" somewhere outside the organism as a relational construct. For the potentiality of the rubber ball to bounce is presumably directly related to the force exerted upon it. The potentiality of the child, in the sense used in the first quotation, seems more nearly analogous to the potentiality of the rolling snowball, growing into ever novel powers and qualities. It would seem to be the case that potential, in this context, moves along the organismic-time-culture manifold. Otherwise, the child

[2]From the point of view of experimental design, this may be simply a plea for additional criterion variables, other than Thurstone's primary mental abilities.

must at every instant be intrinsically different from what he would be if he were not changing.

It is the critical importance of early learning, and the growing awareness of the functional continuity between learning and ability, which make a term like potentiality unsatisfactory in such a context. The problem is always to convert the developmental process, which is a dynamic movement and marked by change, from a fact which is broadly and vaguely apprehended to one which is more precisely determined. But there is always, too, the seemingly irresistible urge in such matters to include the future as a vector in present definition. This accounts for the shifting referents which the word "potential" has, as well as the apparent urging forward of a descriptive term into a predictive term. When Ferguson speaks of children "functioning fairly close to the limit of their potentiality with regard to certain classes of activity," he is probably using "potentiality" as a relative descriptive, having reference to the complexity of our cultural demands on the child in terms of a variety and multiplicity of learning situations, and to a Piaget-type notion of developmental stages.

The term "abilities," too, has an aura of mental actualization about it. Presumably ability should not be thought of apart from performance situations. The instrumentalism implied in this position bears some resemblance to Dewey's formula: learning equals purposive activity. There is some danger that abilities in Ferguson's terms become logically indistinguishable from transfer learning.

The section on human ability and culture[3] is the least carefully specified section of the paper. The general position which Ferguson takes—that it is not feasible to give any account of differences in intelligence relative to differing cultures or stages of culture by means of culture-free tests—is now largely inescapable. He is quite clearly right in this matter. This becomes obvious in the case of the effect language idioms have on reasoning "ability." Evans-Pritchard's work with the Azande is relevant on this point.[4] At the conclusion of his report on the blindness of the Azande to natural causation in the matter of the poison-oracle, he remarks: "They reason excellently in the idiom of their beliefs, but they cannot reason outside, or against, their beliefs because they have no other idiom in which to express their thoughts" (3).

There is a wealth of such data to corroborate the position that at present mental ability cannot feasibly be rendered culture free from

[3]It is not clear from Ferguson's discussion what the boundaries of the term "culture" are, whether in fact it is broad enough to include everything that is, or whether it is conceived more narrowly as "explicit and implicit designs for living" (7).

[4]Malinowsky's work on the Trobriand Islanders might also be cited in this regard

language idiom. Language is one indicator of a wide range of discoverable abilities, which differ from culture to culture, within a learning-ability matrix. As long as ability classifications continue to be specified in terms of *content*, then there must remain this irretrievable fiduciary relation between ability and culture. This trustee relation, however, may be in fact nothing but the inner compulsion of current models. It is equally possible, logically, that "abilities" might be regarded as delimiting boundaries of the thinking process. For example, repetition of a skill may have both a positive and a negative effect. The effect of repetition may be positive in the sense that it provides "content" for thinking, and negative in the sense that it may *delimit* the domain within which thought takes place.

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# PSYCHOLOGY VERSUS MAN[1]

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It is just over 30 years since I heard my first lecture on psychology. I had entered the subject as a graduate student, impelled toward it, like others before and since, by an absorbing interest in human beings, in their thoughts, feelings, and actions, and in the problems of human relations which then, as now, perplexed the world. My concern about human beings had been formulated in religious terms, but I had reached a point where I could no longer accept the assumptions of orthodox religion. Psychology seemed to offer me similar interests and goals, with immunity from questionable doctrine.

Now, 30 years later, I am finding the assumptions of orthodox psychology equally unacceptable. Many different assumptions are of course made by psychologists; those that stick in my throat are, unfortunately, the most congenial to modern science, the most expertly formulated, the most widely employed, the most generally influential assumptions—those of current neobehaviourist psychology.

It is rather late for me to become an expert on mediaeval costume, so I shall probably continue to call myself a psychologist. But during the years that remain to me I shall be trying, ineffectively no doubt, to instil in my students a deeply sceptical and questioning attitude with respect to the most popular assumptions of North American psychology, the meaning of experiments based on them, and the empirical generalizations drawn from them. Having blithely taught a behaviourist social psychology to numbers of students in the late thirties, I feel I should now make some amends, if only to their children.

The assumptions I am referring to are variously stated; the list that follows, based on S.E. Asch (1, Ch. 1),[2] lacks precision and includes certain interpretations:

1. The original motivating forces of human behaviour are visceral drives or needs; these are by definition blind and non-rational.

[1]Dinner address at annual meeting of the Ontario Psychological Association, Hamilton, Ontario, February 4, 1955. Responsibility for publication is, in this case, assumed by the Assistant Editor and a group of consultants.

[2]The author is indebted to Professor Asch's challenging *Social Psychology* at a number of points where formal acknowledgment was impracticable.

2. Any other motives which appear to determine behaviour are second-order drives erected upon the visceral drives through learning.

3. Learning itself consists in the arbitrary association of stimuli with responses, or stimuli with stimuli, and occurs by virtue of the reduction or satisfaction of some primary or secondary drive state. Nothing is required of the learner except that he have some drive state to reduce, and the sensory and other equipment to receive stimulation and make responses. Thus the learning process, too, is assumed to be blind, mechanical, and non-rational.

4. As a corollary, man is basically and inescapably ego-centred. His socially directed acts are habits, learned on the basis of visceral drives or their derivatives, and performed exclusively in the service of these drives.

5. As a further corollary, man thinks, if at all, only instrumentally, as a means to reducing his own ego-centred drives, either by satisfying them more efficiently, or by rationalizing his failures to do so.

I think these propositions are a fair statement of the most general assumptions of current scientific psychology. If you happen to be Freudians, not behaviourists, I would say that the assumptions of Freud's immensely ingenious theory differ only in minor ways; man is still the creature of his organic, egocentric drives; his thought processes solely mediators between irrational drives and harsh reality. If you protest that you are neither a behaviourist nor a Freudian, I am forced to agree with Hebb and Asch that you are a psychologist without a theory.

Now, the first thing to say about these assumptions is that, as assumptions, they are perfectly legitimate. They have a long theoretical history, but their main source today lies, not in observations of human behaviour or in speculations about it, but in a methodological requirement —the need to simplify experimental situations by reducing the number of variables. The effects of this need can be most clearly seen in current learning experiments. As Donald Snygg has said:

> No matter what apparatus is used, it is sure to be one which limits the possible behavior of the subject to two simple alternatives at each choice point. This is done to simplify and facilitate record-keeping. Then, so the animal will not learn too fast, all but one of the possible cues that it might use for solving the problem are eliminated. Personality differences are further minimized by arranging that all subjects share a common physiological tension, usually hunger, during the experiment. Such practices would make no sense if the experimenters were trying to learn about people or even about rats. But they follow quite logically if the experimenter is trying to study a process [9, p. 132].

In other words, both theoretical assumptions and experimental practice served, originally, a purely practical and heuristic purpose, and that

is still their status wherever a good experimentalist is concerned. Unfortunately, however, in the rapid expansion of psychology on this continent, good experimental thinkers have been vastly outnumbered by —well, other psychologists. Regiments of uncritical teachers and text-book writers, having picked up the ABC of behaviourism from Watson and his successors, stormed the psychological supermarket in the laudable determination to make their courses scientific. They filled their shopping-bags with selected experimental findings, and spread these later on the pages of their books and the air of their class-rooms.

What they seldom noted, as they sorted out their merchandise, were the precise and limiting conditions to which the findings were tied; what they practically never appreciated were the logical relations between initial assumptions and findings. Often, in fact, we seem to have believed—for I include myself in the indictment—that a successful experiment "proves" all the assumptions under which it is conducted. I hesitate to impute so much naivety to a learned profession, but how else can we account for the fact that the parsimonious assumptions within which experimenters have chosen to work have so often become, a little later, the facts which psychology tells us about human beings?

Even fairly significant experiments have been grossly misinterpreted. Watson and Raynor's classical experiment in conditioning a child to fear a rabbit did show that an 11-month child could acquire a fear-reaction in that blind, non-rational way. It did not, of course, show that all fears are due to conditioning, let alone that cognitive processes play no part in emotions. Yet these are the interpretations which numbers of students have carried away. As Asch acutely inquires (1, p. 100): Why did the experimenter stand *behind* the child when he hit the bar? If even an 11-month child had seen the experimenter hit the bar, would he have come to fear the rabbit—or the experimenter?

Our inability to be critical in these matters, or to see any difference between a low-order empirical law and a scientific truth about human nature, may be blamed on defects in our training. But it was certainly encouraged by the rash generalizations of the early theorists themselves. Watson himself was a shining example, and many of our text-books are not far beyond Watson. There is no excuse for such confusion today. Spence has recently stated: "The data heretofore investigated by the theory-orientated learning psychologist . . . consist in observations obtained under controlled laboratory conditions that often depart radically from those of every-day life. . . . The laws that the experimental learning psychologist has discovered . . . grew out of these laboratory phenomena and as yet have not been related to instances of learning in 'real' life" (10, pp. 7, 2). Psychologists whose assumptions

about human behaviour are less simple and mechanistic than those I have listed need not be intimidated by experimental findings, once they have learned enough about scientific method to understand the complicated game which the experimenter is playing.

However, although the propositions I listed are properly working assumptions, there is no question that the leading workers within this theoretical framework expect their formulations to be ultimately applicable, with necessary modifications, to the whole range of human behaviour, from Al Capone to Albert Schweitzer. Notice what Spence says: they are not *as yet* applied to real life. Not by him, perhaps, for he is a cautious scientist, but they *are* being applied, and in many areas. Why does a child seek his mother's attention? Miller and Dollard tell us: because he has learned that "when mother is following him with her eyes, good things are likely to come. To have her look at him . . . is a stimulus which promises reward rather than trouble" (6, p. 140). This is an untested generalization, and in its present form, untestable. How do we become attached to persons, groups, causes? Mark May tells us: "Most people learn to love those persons, objects, places, and conditions which are associated with less hunger, less thirst, less pain and fatigue, or more money and prestige" (5, p. 51). That is presumably how Schweitzer, the renowned musician, theologian, philosopher, came to devote his life to the West African natives. On every hand, assumptions which have survived a purely negative test, at an artificially simplified level, are appearing as generalizations in areas where they have not been tested at all. And with practically all the studies in the experimental journals now devoted to aspects of behaviour theory, its initial assumptions are bound, through sheer popularity, to transcend their original limitations and be transformed into a doctrine of human nature. That is what disturbs me.

I have sketched, very roughly, how theoretical assumptions, made for experimental purposes, are tending to become the accepted statements of how psychology regards man. Whatever their status, however, the interesting and perhaps encouraging fact is that none of us thinks of applying them in the everyday life which still occupies a few moments of each day. We may do so to a limited extent when we are dealing with children, or with adults whose behaviour is disorganized; and they are applied, with apparent success, by politicians, advertisers, and other propagandists—a point which we will deal with later.

Not one of us, however, makes such assumptions in dealing with normal, adult human beings, particularly those to whom we are bound in any significant social relations. On the contrary, we assume that they will usually subordinate their primary, visceral needs to the allegedly

secondary motives thought to be born of them; that they have the capacity to understand their situation, and to act in terms of their understanding; that they are nearly always willing, say, to push a stalled car without specific reward; and that they carry within them notions of fairness, reasonableness, justice, and truth which play a significant part in guiding their behaviour.

In other words, we psychologists are in a paradoxical position not shared, so far as I know, by any other scientist. Outside our laboratories we deal with our subject-matter—students, families, friends, patients,—as if these creatures possessed properties of rationality, unselfishness, and self-determination which are explicitly denied them by the theories we profess to hold.

This schizophrenic behaviour might be passed over as just good, clean, academic fun, if it were not for two considerations on which I want to enlarge a little. The first is frankly value-orientated; it has to do with our relations, as psychologists, to those outside our ranks. The second concerns the growing influence of the behaviourist movement I have referred to.

Teachers, clinicians, and all the rest of us, are members of a recognized professional group; as such we have certain social responsibilities which we cannot evade.

Those of us who teach have responsibilities to our students, not only to those who are going to join our profession, but to the far greater numbers who take courses in psychology as part of their liberal education. The defining of a liberal education has puzzled wiser heads than mine, but one of its central features has always been this: that the student is brought into contact with the best that man has achieved or sought to achieve on the earth; with the astounding variety of social forms which man has constructed, each one a complete design for living; with his unquenchable determination to explore and understand his surroundings, geographic, physical, and social; with his amazing creative achievements, from Gothic cathedrals to walrus-tooth carvings, from Chaucer to Dylan Thomas, from Bach to Britten, from Leonardo to Picasso. We assume that where the student has seen and felt the best, whether in science, art, or literature, he will sometimes choose the best. We also try to convey something of man's tireless search for the meaning of his existence; of the social goals and values which have guided his conduct; of his undying belief that in spite of failures, betrayals, wars, and bloodshed, he could live fully and happily on earth, and in peace with his fellows. A liberal education should of course leave us realists, well aware of the historic failure of man to realize his aspirations, but also with a humble but inspiring sense of man's potential greatness—the feeling

which called forth Shakespeare's apostrophe: "What a piece of work is a man! How noble in reason! In action how like an angel! In apprehension how like a god!"

That has always been one aspect of liberal education. With the twenties, however, there began an immigration of psychologists into the Arts faculties—harmless-looking people, good colleagues, husbands, and fathers, but with a queer inner rigidity which forced them to insist, when pressed, that no man ever did anything at all which did not contribute, directly or indirectly, to reducing tensions in his belly or his gonads. What have we to say about someone like Albert Schweitzer? If we are honest with ourselves, little or nothing. But if we are honest with our theories, we must say that his devotion to the music of Bach is the end product of a fortuitous chain of habit acquisitions; that his moving words about the religious meaning of life are probably rationalizations of his own inner weaknesses; that his 40 years of devotion to sick natives in Lambaréné are merely a disguised form of self-gratification.

An appreciation of scientific method is an essential part of a liberal education today, and psychology should certainly contribute to it. But to pass out, even by implication, such half-baked assumptions as if they were the scientific truth about human nature—this seems to me an attack upon man himself which would justify kicking us out of the Arts faculties altogether.

In practice, of course, we give our students no such picture. In the introductory course we make a bow to scientific determinism, gloss over the mechanistic assumptions of learning theory, and then go on happily to social motives, social adjustment, personality integration, insight, empathy, mental health, as if no such assumptions had ever been made. We are usually aided by the text-book, which does the same thing. Only rarely are we asked an awkward question, for this procedure of ours has effectively destroyed the students' capacity for logical deduction. Psychology, however, comes out as, on the whole, a "good thing." How much longer will we be able to do the splits like this? Not long.

We also have responsibilities to our colleagues in other disciplines. It has often been said that, just as physics provides a touchstone for all the natural sciences, informing them what matter is ultimately like, so psychology should be able to tell the social sciences what man is really like. We seem less able to do this than ever before; Freudian theory has its anthropological addicts, but scientific psychology is becoming more and more irrelevant to the problems of human behaviour in an organized social context. As Snygg said, it is no longer concerned with man, but with abstract processes. We will see shortly where this takes us. As for our colleagues in the humanities, who *are* interested in man—well, let

Gardner Murphy speak for me:

> Those who think in terms of folklore, literature, painting, music, pageantry, the dance, and the theatre . . . these people glance at our psychologies of motivation and turn away disheartened. We list for them a series of basic biological motives. They have heard of these things before; they do not quite see what is involved in listing them. Often they want a bridge built from the world of their existence to the technical psychology of today; they not only find no bridge but often a studied attempt to deny that a bridge needs to be built [7, p. 613].

We have, finally, a duty to society in general, for our views about man are steadily penetrating our culture. We may protest that we are concerned only with the truth, whatever its social consequences; but let us first be quite sure that we know the truth. In the meantime, doctrines which make man the passive plaything of forces beyond his personal control, and largely beyond his ken, do untold social damage by seeming to give scientific confirmation to the very notions which democratic societies are struggling to combat. When we assert the absolute primacy of narrow self-interest, we justify many of the worst practices of our political and economic systems. When we imply that human beings will work and learn only under the threat of deprivation, we are backing the old-style employer against all the industrial psychology since Hawthorne. When we imply that man does not think for himself, and can be manipulated to any given extent by arranging the stimulus conditions, we are not only vindicating the whole smooth structure of modern advertising, but putting ourselves on the side of Hitler, Stalin, and their imitators, and against Thomas Jefferson and all who have believed that man is worthy of freedom.

There is of course some truth in all these notions, or they would not have achieved currency, but we psychologists should be pointing out the narrow limits within which they are valid, not asserting their universality. Men *do* act stupidly, they *can* be manipulated by advertisers and demagogues. But they are also trying constantly, to the best of their ability, to make sense of their surroundings, to understand their situation, and to act as the situation demands. And when their information is adequate, they do so act in most cases. Suppose someone advertises: "INCREDIBLE bargain in Television! HUGE 14-inch SCREEN! Complete with Super-Animator, Ultra-Vutoner, Electronic Wave-Deflector! Only $49.95!" It will work; crowds will show up. But does that prove man an irrational being, moved only by blind cupidity? Of course not, and further, it strongly implies the contrary. For this ad is successful precisely because it duplicates the learning experiments described by Snygg; it is almost a replication of Watson's fear-conditioning experiment.

Remember Asch's question: "Why did the experimenter not let the child *see* him bang the bar?" Because if he had, it wouldn't have worked so well. And why did the television merchant not say in his ad: "Of course, these sets are constructed of reclaimed materials, the picture tubes are seconds, there is no guarantee, and I have neither the facilities nor the intention of rendering any service?" Because the ad wouldn't have worked either. The implication in both cases is the same—that human behaviour is guided by cognitive factors, by reason; but both are interpreted to mean precisely the opposite.

It is true, again, that most people act with an eye to their own self-interest, and that for some the world is literally ego-centred. But again, that is not all. They also show an objective interest in their surroundings, they like to watch machines operating, to listen to music, to work at crossword puzzles, to lose themselves in a good play. And this outward, objective orientation reaches its climax with respect to other human beings. They are foci of life-long interest and concern; we attach high value to them, we are strongly motivated by our relations with them, and our abstract values—truth, justice, fair play—all derive from these social experiences. Such behaviour lies outside the scope of any mechanistic theory, and cannot plausibly be brought within it.

Take the simplest sort of example. A few months ago a Toronto paper reported a working mother who lost on the streetcar a wallet containing $45, her whole week's pay. The story was not featured, but at once contributions started coming to the newspaper, many of them anonymous; $2 from an old-age-pensioner. What kind of behaviour is this? A habit, acquired through frequent reinforcement? Suppose you look, not at a text-book discussion of habits, but at what is known experimentally about them; you cannot take care of that old-age-pensioner with all the generalization gradients in Iowa. Or do I hear a Freudian murmuring "sense of guilt?" If you honestly believe that, you are beyond hope. I say these people felt *sorry* for the woman, and if you do not know what feeling sorry for someone is like, then I feel sorry for you. And if giving such emotions a place in the behaviour sequence makes you feel scientifically insecure, read the current work of Leeper, Goldstein, Nuttin, and Asch, and you may feel better. As Scheerer says, "Emotions may well turn out to be the unique expressions of the unity of cognitive and conative functioning" (8, p. 122).

No, we are not pulling our weight in the struggle to formulate a doctrine of man that can be opposed to that favoured by the dictators. Democracy is crying out for one, but orthodox psychology has nothing to offer. Indeed, we harmless-looking psychologists are carrying in our

briefcases a time-bomb potentially more destructive to democratic ideas than any hydrogen bomb can be. The only reason we have not yet been detected is because of the despised applied psychologists, the clinicians, human relations boys, mental health people, who are working constructively *for* human beings, *for* society, but on assumptions totally at variance with those of scientific psychology.

This ambiguous situation, however, will not last. It will not last because of the intellectual and technical skill of the S-R reinforcement psychologists, the immense prestige and increasing momentum of the movement toward an exact, natural-science psychology. That movement is already so powerful on the theoretical and experimental fronts that in ten years it will *be* psychology—if it has not changed its name by then. I sat down to lunch the other day with two colleagues, apparently astronomers. One was saying to the other: "Before we knew anything about science, we called our subject astrology. Then, when we became scientific, we changed its name to astronomy. Now, of course, its becoming astrophysics." Then, turning to me, he said: "You're in psych*ology*, aren't you?" Perhaps "psychonomy" is what we are coming to. However that may be, in ten years' time I think it will not be possible for text book writers and teachers to continue doing the intellectual splits; the initial mechanistic assumptions will have to be carried right through the course. I am glad I shall not be teaching then.

Do not misunderstand me. I have the greatest admiration for the learning theorists. I admire their complete dedication to scientific enterprise, I admire the clarity and openness of their formulations. And it is admiration untinged by emulation, for I know myself incapable of the rigorous thinking and unbelievably strenuous work they are doing. I think it is misdirected work, but it is undoubtedly the best scientific work being done in psychology today. Can we then leave them to carry on, assured that the mechanistic theories will be modified as more facts are taken into account?

There are two things to note in this connection. One is that the only experimental findings which will have any effect on these theories will be those secured within the limiting framework of neopositivism, with its rejection of hypothetical constructs and its insistence on operational definition. Hence a vast amount of work usually regarded as psychological investigation will be dismissed as irrelevant. On the other hand, acceptance of these limitations, derived as they are from the logic of the physical sciences, will, I suspect, preclude from the start the demonstration of any aspects of human behaviour not found, say, in an electronic calculator. So the dice are, in a sense, loaded to start with.

And secondly, wherever acceptable findings appear to conflict with some aspect of the theory, ingenious and strenuous efforts will be made to assimilate them or argue them away. These are legitimate tactics in scientific controversy, but they do not make theories more readily modifiable. For instance, the motivational side of S-R theory is notoriously vulnerable, particularly as regards the assumption that all motives are derived from tissue needs. But Harlow complains: "The fact that derived drives based on homeostatic needs are unstable and transient, the fact that the conditioned drive stimulus does not apparently reinstate the unlearned drive state, and the fact that human beings learn and live for days, weeks or months without or in spite of a particular homeostatic need state, do not disturb such psychological theorists in the least" (2, p. 37).

Harlow's own work on the externally elicited drives of exploration and manipulation is a case in point. His results are well confirmed, but already efforts are being made to assimilate them by postulating another internal drive state, akin to Blatz's "appetite for change." The reason is clear; admission of the perceived world as a valid source of motivation cannot be squared with the drive-reduction principle; it would release man from egocentricity and open the door to social motivations of a primary, not secondary, sort, which would readily cover such cases as those of our old-age-pensioner and Albert Schweitzer.

We do seem at last to have got experiments on relational learning which cannot be adroitly reinterpreted in terms of absolute stimulus values and reinforcement. Response to perceived relationships is at least a step toward putting cognitive processes back into the behaviour sequence. But I shall be surprised if the learning theorists do not find that this too can be taken care of. There is much truth in Conant's remark that no theory is ever overthrown by contradictory facts, but only by a better theory.

And there's the rub. The exponents of man as an elaborate machine have a theory, while we tender-minded psychologists have none. Look at the unhappy confusion of the chapters on cognitive theory and field theory in Lindzey's *Handbook of Social Psychology* (4), and you will understand why the precise, unequivocal formulations of Hull, Spence, and their colleagues have such enormous drawing power. We are losing this game by default. Construction of a competing theory will be a long and arduous process, requiring intense intellectual effort, and even the foundations are not really laid. Asch has given us invaluable guide-posts and suggestions, but most of us are content to float along with a miscellaneous set of *ad hoc* formulations—a bit of Gestalt, a few organismic ideas, a spoonful of Freud, and a pinch of Carl Rogers. I think there is a

challenge here to our younger psychologists as stimulating as any ever presented, and I hope some of you will respond to it.

Personally, however, I doubt whether one can formulate a psychological theory fit for man without first re-examining some assumptions far deeper than those we have been talking about. Wundt, they say, founded scientific psychology, and scientific psychology is still the watchword, at least on this continent. My own conviction is that a scientific psychology, in the present rigorous definition of that term, *must* lead to a psychology of abstract processes, a psychology with man left out. An experimental psychologist, G. A. Kimble, has pictured this ultimate development as follows: "For all practical purposes, it is quite possible to construct a science of psychology in which the organism is considered as empty. For my own part, I can conceive of a psychology based on stimulus and response events entirely, one in which the existence of the organism is a completely unimportant fact" (3, p. 158). That is a logical goal, a legitimate goal, and a fascinating goal to pursue. In the light of it, the apparent blindness of the learning psychologist to obvious human characteristics becomes entirely understandable. But is that the only goal psychology can have? If so, we poor ninnies who came into psychology because we were interested in human beings will have lost our subject-matter, or else be accepted as psychologists in the purely "astrological" sense. We have only ourselves to blame, however, for the nemesis that has overtaken us. We had to be scientific; now we begin to see what that means. We still announce to our classes that the aim of psychology is the prediction and control of human behaviour. But when we say prediction and control we are making, I fancy, the very assumptions about our subject-matter that lead without a logical break to Kimble's vision.

In such a dilemma (and it is a real one) each must choose for himself. My own choice is probably already apparent. I accept determinism, yes; but with the proviso that an essential phase in the determination of behaviour occurs in the cognitive organization of the individual. And in that phase I myself am involved; I am in the process, I am a part of it. Properly understood, that is the psychological experience of free will. I accept the scientific attitude of unbiased search for truth with all the resources of the intellect, but I question whether human behaviour at its complex levels will be fully revealed by the present methods of the physical sciences. Prediction? Here I go back to Cooley's wild grape vine. We can predict in general its habit of growth, but no one can predict the precise form which each branch will take as it clambers along the rail fence. Control? Yes, again, in a general sense; we *can* improve the vine by water and fertilizer, and the human being by wise handling

in childhood. But control in the *full* sense? Have you ever thought about it? That is an aim which I trust no psychology will ever achieve. What a priceless gift to the dictators of the future!

On such a modest, old-fashioned basis, with the investment of untold intellectual effort, I think those who want one may ultimately construct a human psychology. It will be scientific only in the sense that, say, agriculture is scientific; but it will be *for* man, not against him.

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# ASSOCIATIONAL FLUENCY AS A FUNCTION OF STIMULUS ABSTRACTNESS

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IN A RECENT paper, Cofer and Shevitz (1) analysed the relationship between fluency of verbal associations and the commonness or rarity of stimulus words. Reliable differences were found between the numbers of associations elicited by stimulus words which varied in commonness as determined by word-frequency counts. The importance of the Cofer and Shevitz study, like that of Haagen (2), lies in the fact that it gives clearer definition to properties of verbal stimuli. The present study considers an additional dimension of stimulus variability. Here the commonness-rarity difference is held constant and stimuli are chosen which vary along an abstract-concrete dimension. Both Jung (3) and Woodworth (10) have noted that association time (delay between presentation of the stimulus word and the subject's response) is usually shorter for "concrete" nouns than for either "abstract" nouns or adjectives, although their definitions of abstract and concrete are not made explicit. Accordingly, the hypothesis to be examined here is that concrete nouns will be more provocative as stimulus words, i.e. will elicit more associational responses, than abstract nouns or adjectives.

## METHOD

### *Subjects and Procedure*

It was practicable to test the hypothesis using subjects who had volunteered for another study, see Lambert (4). One sample comprised 28 college-trained men and women who were either undergraduate or graduate students in Arts and whose native language was English. The second sample was made up of 14 French natives all of whom had had university training in Europe at least through the B.A. degree with specialization in Arts. Thus, the relation between stimulus provocativeness and stimulus abstractness could be studied in two languages, using native speakers as subjects.

Subjects were tested individually, the task being to give continuous associations (single words, not phrases) to stimulus words for 45 seconds. Stimuli were presented in a Ranschburg exposure apparatus and the subject was instructed to say aloud "all single words which come to mind as you look at and think about" the stimulus word. Subjects were told that



CANAD. J. PSYCHOL., 1955, 9 (2).

only a linguistic and not a "clinical" analysis would be made of their responses. The responses were recorded on tape, and the experimenter kept a continuous tally of the number of responses in each five-second period after the exposure of the stimulus word.

*Stimulus Words*

All stimuli were assembled from the most frequently used categories as determined by Thorndike and Lorge (**6**) for English words and Vander Beke (**7**) for French. Stimulus words were selected which appeared free from emotional or clinical implication—the only restriction put on their selection.

Criteria for concrete and abstract were as follows: if the referent of a stimulus word was a touchable or manipulable thing, the stimulus was considered concrete; if not, it was considered abstract. According to this interpretation, adjectives differed from abstract nouns only in that they are qualifiers, for adjectives are equally abstract in terms of their referents. Stimulus words are listed in Table I.

TABLE I

FRENCH AND ENGISH STIMULUS WORDS

| Concrete nouns | | Abstract nouns | | Adjectives | |
|---|---|---|---|---|---|
| ami | child | esprit | honor | grand | dear |
| argent | food | idée | idea | jeune | little |
| main | garden | jour | peace | juste | rich |
| maison | house | temps | thought | libre | strong |

To counteract practice and fatigue effects, the stimulus words, in both languages, were presented in a balanced order; for example a concrete noun followed by an adjective at the start of the test period would be balanced by another concrete noun preceded by an adjective at the end of the test period. Responses to like stimuli (e.g. the four concrete nouns) were then totalled for all comparisons.

## RESULTS

The hypothesis was clearly substantiated in both languages. Table II shows the cumulated average number of associational responses given by the subjects for the three classes of stimuli, during periods of 5, 15, 30, and 45 seconds. When tested at these time points during the association period, the concrete nouns elicited reliably more responses than did the abstract nouns or the adjectives, but there was not a reliable difference between the abstract nouns and adjectives. Table III sum-

TABLE II

MEAN NUMBER OF ASSOCIATIONS

(Each entry is the cumulated average number of associations given by subjects at several points in time during the association period.)

| Time in seconds | English language ($N=28$ subjects) | | | French language ($N=14$ subjects) | | |
|---|---|---|---|---|---|---|
| | Concrete nouns | Abstract nouns | Adjectives | Concrete nouns | Abstract nouns | Adjectives |
| 5 | 1.92 | 1.49 | 1.46 | 1.68 | 1.30 | 1.46 |
| 15 | 6.12 | 4.40 | 4.38 | 5.91 | 4.91 | 5.14 |
| 30 | 10.72 | 7.83 | 7.78 | 11.04 | 9.36 | 9.38 |
| 45 | 14.62 | 10.82 | 10.47 | 15.46 | 13.46 | 13.05 |

TABLE III

PROBABILITY ($p$) OF A CHANCE OCCURRENCE OF DIFFERENCES

(From Wilcoxon's (9) table for comparisons of paired associates.)

| | English language ($N=28$ subjects) Time intervals | | | | French language ($N=14$ subjects) Time intervals | | | |
|---|---|---|---|---|---|---|---|---|
| | 5 | 15 | 30 | 45 | 5 | 15 | 30 | 45 |
| Concrete nouns—Abstract Nouns | $<.01$ | $<.01$ | $<.01$ | $<.01$ | $<.01$ | $<.01$ | $<.01$ | $<.01$ |
| Concrete nouns—Adjectives | $<.01$ | $<.01$ | $<.01$ | $<.01$ | $<.03$ | $<.03$ | $<.01$ | $<.01$ |
| Abstract nouns—Adjectives | $>.05$ | $>.05$ | $>.05$ | $>.05$ | $>.05$ | $>.05$ | $>.05$ | $>.05$ |

marizes the tests of significance between the mean frequencies at the four points in time.

## DISCUSSION

Cofer and Shevitz were unable to account for the finding that adjectives elicited reliably fewer associations than did nouns when the nouns and adjectives were of the same word-frequency rating. Their finding, however, is clearly consistent with the difference between concreteness and abstractness established here.

A more complete description of data becomes possible if account is taken of stimulus abstractness. For example, White (8) asked subjects to give continuous associations to stimulus words chosen to represent

sad and happy experiences. He found a larger mean number of associations to the "happy" stimuli, and concluded that happy experiences are more meaningful and intense. On analysis, however, it appears that White's "sad" stimuli included comparatively more abstract words which alone could account for the differences found.

The finding established here may prove useful by increasing the precision of selection of verbal stimuli for use in such areas as verbal learning or association-time analysis. For example, concrete words might prove to be more quickly memorized and longer retained than abstract ones. Before clinical significance is attributed to slow reactions to particular words, the abstractness of these words should certainly be considered.

A recent investigation by Noble (**5**) defines "meaningfulness" in terms of richness of association. A stimulus word which elicits a greater average number of associations is more meaningful than another eliciting fewer associations. It appears that Noble's meaningfulness is determined by at least two variables—stimulus commonness according to Cofer and Shevitz, and stimulus abstractness according to the present study.

## Summary

Free associations were secured from 42 subjects to a list of familiar nouns and adjectives. All words had equal word-frequency count ratings, but half the nouns were abstract, half concrete. Reliably more associations were made to concrete than to abstract nouns or to adjectives. There was no difference between abstract nouns and adjectives. Some research implications are discussed.

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# EXPERIMENTAL INVESTIGATION OF BEHAVIOUR IN SOCIAL SITUATIONS:

## I. BEHAVIOUR UNDER OPPOSITION[1]

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THIS is a report of the first of a series of experiments on personality and social correlates of the social behaviour of individuals. It outlines briefly the theoretical framework of the project and reports on the "opposition" situation, in which each subject had to debate a topic with two others, both of whom, by previous arrangement, uncompromisingly opposed his opinion.

The experimental situations were designed on the basis of a provisional theory of social behaviour, which includes concepts from Eysenck (**5**) and the writer's previous investigation (**1**), translated into the generalized framework of Hullian behaviour theory (**7, 14**). This generalization of behaviour theory is regarded as a first approximation to an eventual theory of the social behaviour of individuals, verification of which is being sought through testing of hypotheses deduced from it (**4**). A formal statement of the theory has been prepared in mathematical form and will be published when sufficiently matured. In this paper, only a brief outline of relevant concepts is given to indicate how situations were constructed and predictions derived. Complete derivations of hypotheses cannot be given without statement of all assumptions, which is not practicable here.

### OUTLINE OF THE THEORETICAL FRAME OF REFERENCE

The performance of a person in a group situation is regarded as a function of his personality traits and of the behaviour of other group members. It is assumed that a social situation, and the behaviour of

[1]Research done under National Health Grants, Project no. 605-5-124 (to Professor J. D. Ketchum).

[2]The writer is indebted to Professor J. D. Ketchum for suggesting the general area of research, certain situational settings, and the use of role-playing assistants, and also for advice and help throughout the experiments. Sincere thanks are due to those who kindly read this paper in draft: Professors J. W. Lovett Doust and H. A. Shephard, Mr. M. Humphries, and particularly Dr. R. B. Bromiley, who is responsible for many crucial improvements in the text; to the students who served as assistants and observers; and to Joan Cervin for editorial help and typing.

a person in it, can be described sufficiently (for a particular purpose) in terms of a few social and personality dimensions or variables.

The following variables are relevant for our experiments:

1. *Introversion-extraversion* of subjects, the extremes of which Eysenck terms dysthymia and hysteria (5, p. 51). It may be characterized by the level of manifest anxiety exhibited (3, 15). This variable is interpreted as the *generalized drive* ($D$) of behaviour theory (17, 19). Operationally, it may be defined, e.g. by an anxiety test score (19), or it may be produced experimentally.

2. *Eysenck's P-dimension* (personality organization), called neuroticism at one end (5, p. 49), and extended here, in contrast to Eysenck, to include underorganized, labile personality (e.g. psychopathic) at the other. The degree of personality organization is interpreted in terms of *degree of interference* of strongly generalized, irrelevant response tendencies ($\bar{H}$) with those ($H$) relevant to a situation (8, 13). Interference can be expressed as an algebraic difference between habit strengths (8, p. 66). It results in a deterioration of the appropriate responses (errors), a phenomenon observed by learning experimenters (6, 11, 13, 20), clinicians (15), Rorschach technicians (10), and others (2). Interference can be operationally defined, e.g. by the deterioration of performance on an intelligence test (15), or directly by paper-and-pencil or projective test scores.

3. *Emotional instability* is conceived as a complex state in which anxiety and neuroticism play important roles. It is conceptualized as the *initial reaction potential* ($E_o$) with which subjects enter the experiment, i.e. it is a function of drive and differential habit strength (7). Operationally, it can be defined by a paper-and-pencil test score (12).[3]

4. *Solidarity* (1) or dissolidarity is the degree of group approval or disapproval of a subject's performance in relation to a common task. It is conceptualized as positive or negative *reinforcement* ($\pm K$). It can be experimentally controlled by prearranging the behaviour of other group members. This prearranged behaviour serves as its operational definition.

5. *Attitude of the group* toward a member, i.e. acceptance-rejection of his person, is conceived as positive or negative secondary *reinforcement* ($V$) of his ego-attitude. This variable is defined by operations similar to those defining variable 4.

6. *Compatibility* (1) or incompatibility of responses available to a subject in a situation is the degree to which these responses are mutually

[3]For technical reasons, emotional instability, rather than anxiety or neuroticism, was the variable measured in the experiment reported here. We are at present attempting to measure variables 1 and 2 directly.

exclusive. It defines for him the *complexity* ($X$) of the situation from the point of view of possible competition between responses. Incompatibility arises, for instance, when a subject has no possibility of correcting his responses (18); such responses are mutually exclusive and the situation is then termed complex. Number of responses and their compatibility can be operationally defined by the structure of the situation as arranged by the experimenter (16).

The above intervening variables represent the independent experimental variables, manipulable by the experimenter.

The writer assumes that behaviour tendency is a direct function of the intervening variables 1 to 5 and an inverse function of variable 6. It can be predicted in terms of effective reaction potential: $E = f(E_o, K, V, X^{-a})$. In this formula $E_o$ serves largely as a constant. An observable response ($R$) is coordinated to its reaction potential ($E$) as a particular case to a concept. Observations of responses were made in terms of their latency, amplitude, proportion of shifts of opinion, and voice level, as described in the Procedure section of this and the following papers.

## Design of the Debate Experiment Characterized by Opposition

The experiment was designed to vary variable 3 and to hold variables 4 to 6 constant. Variation in personality of subjects, variable 3, was achieved by selection of one sample of subjects with high and one with low initial reaction potential. Subjects with high initial reaction potential will be referred to as the experimental sample, those with low as the control sample. Both experimental and control subjects were exposed to identical experimental situations.

The situation of opposition was conceptualized as follows: subject's ego-attitude received a slight negative reinforcement (mild threat of rejection from the group) in a situation offering the possibility of at least two incompatible responses (opinions). The dominant one, i.e. the subject's own opinion, was not reinforced, whereas the opposite one was strongly reinforced. Habit strengths of "relevant" responses (opinions) were, it is hoped, made about equal for all subjects by randomly choosing for discussion only topics of general interest, such as the value of the General Arts Course.

In order to derive hypotheses concerning the behaviour of subjects in our groups, certain assumptions were made about the values of variables and the ways of combining them in constructing the situations. For instance, owing to the nature of the screening tests used, the experimental sample should contain both "dysthymics" and "hysterics"(12)

who are in some respects opposites.[4] As their test scores are comparable, it is assumed that hysterics are low on manifest anxiety (drive) and high on neuroticism (interference); dysthymics the reverse.

With these and other assumptions, it was expected that:

1. More experimental than control subjects should persist in their original opinions, since the experimental subjects entered the situation with high initial reaction potentials behind their opinions. The derivation of this prediction is based on the principle of Hull's case IV (9, p. 31).[5]

2. There should be no difference between mean response latency of experimental and control subjects. This prediction follows from the assumption regarding the heterogeneity of the experimental sample: when incompatible response tendencies are elicited, the latency of stating an opinion should be longer for dysthymics (high drive) than for the controls, and shorter for hysterics (low drive) (18, p. 18), especially if noncommittal, defensive statements are made first.

3. Variance of the response latency distribution of experimental subjects should be larger than that of the controls. This is a corollary of prediction 2.

Thus the derivation of predictions of behaviour in an opposition situation resembles that of predictions regarding learning in complex situations. It will be appreciated that either assumption concerning latencies of response of dysthymics and hysterics would lead to the same predictions (2, 3); the resulting indeterminacy can be removed only by testing our assumptions in further experiments, in which dysthymics and hysterics are separated.

## Procedure

*Subjects.* Students of two Psychology classes were screened for emotional stability on three paper-and-pencil tests suggested by J. W. Lovett Doust: the Maudsley Medical Questionnaire, Crown Word Connection List, and Emotional Immaturity Questionnaire (12). Scores from these tests were weighted for their correlation with a psychiatric classification (the *r*s were respectively: .59, .36, and .62) and combined to give the emotional instability scores. The distribution of the 483 screened students on the emotional instability test was peaked and symmetric, and ranged from 14 points ("emotionally stable") to 75 points ("unstable") on a scale of 152 points. Thirty-two extreme cases, with instability scores above 59 (mean 64.7), were taken as experimental Ss; 32 controls, with scores below 40 (mean 27.8), were matched to the upper 32 on several variables (see Table 1). The difference in mean

[4]The clinical terms are used solely for convenience; psychiatrically speaking, all subjects were "normals." The derivations are based on Ss' relative positions on the various dimensions.

[5]In the case of hysterics, it is assumed that defence mechanisms should interfere with their responses, especially the weaker (non-dominant) ones, with results similar to those for dysthymics.

scores between experimental and control Ss was significant ($p < .01$). The sample should be regarded as incidental, randomly selected from a hypothetical universe with features as described in Table I.

TABLE I

SAMPLE CHARACTERISTICS AND MATCHING VARIABLES

| Characteristics | | Subjects | |
|---|---|---|---|
| | | Experimental | Control |
| Number of Subjects | | 32 | 32 |
| Sex | Males | 12 | 12 |
| | Females | 20 | 20 |
| Age | 19–25 | 32 | 32 |
| Marital status | Single | 32 | 32 |
| Citizenship | Canadian | 32 | 31 |
| Mother tongue | English | 32 | 32 |
| Parents living together | Yes | 27 | 26 |
| | No | 5 | 6 |
| Number of siblings | 0 | 7 | 7 |
| | 1 or 2 | 21 | 21 |
| | 3 or more | 4 | 4 |
| Course year | Second | 18 | 19 |
| | Third | 14 | 13 |
| Annual family income | Less than $2,000 | 2 | 0 |
| | $2,000–$5,000 | 20 | 18 |
| | More than $5,000 | 10 | 14 |

*Control of social variables.* Each subject met at the laboratory with two role-playing assistants (of his own sex and strangers to him) who posed as subjects. The same two assistants were used with both members of each matched pair of subjects. The groups of three thus formed were instructed to fill out individual questionnaires, stating their opinions on several topics. These were collected, and during another task the assistants' questionnaires were altered by the experimenter so as to conflict with S's stated opinions. All were then returned to their writers and the groups instructed to debate a specified topic with the aim of reaching consensus. "If no common opinion can be worked out, the group may decide to disagree" (operational definition of variable 6). Assistants had had previous training and were told: "Take the initiative in expressing your opinion from the word 'go' . . . this opinion will be the opposite of that of S" (operational definition of variable 4). "Stick to it obstinately and try to generalize it as much as possible . . . using such expressions as 'everyone nowadays' etc." (operational definition of variable 5). "If S agrees with you, suggest closing the discussion . . . if he disagrees, try to make him change his mind . . . work for a group decision as early as possible." In order to stimulate Ss to good performance, groups were told that they would be watched, timed, and their arguments recorded.

*Apparatus and measurements.* All sessions took place in a demonstration room separated from an observation room by a one-way vision screen. Voices could be transmitted both ways between the two rooms. There were two observers, one operating the apparatus, the other recording time and other variables. The discussion was completely recorded on tape. Performance was measured as follows:

(*a*) Shift of S's opinion was recorded at the end of discussion by the observer as "complete shift," "qualified shift," and "no shift." Reliability was checked against the tape recording; no errors were found.

(*b*) S's response latency, i.e. time until S made his first statement (of opinion) after the word "go," was measured by stop-watch and recorded. Reliability against a recheck from the recordings was .94.

(*c*) "Involvement" of Ss in the situation was rated as present or absent by an observer; no reliability measure was made.

(*d*) Voice level of Ss was recorded from a VU-meter attached to the amplifier. A zero for each S was obtained before the discussion by having him read a short neutral paragraph into the microphone. Variations from this zero were recorded by an observer (ignorant of S's stability score) each time S spoke, and averaged to give S's score on voice level. Reliability, checked as in (*b*) above, was .92.

*Other controls.* In each case the same experimenter read the instructions, experimental material was identical, and, with a few exceptions, the same pairs of assistants always worked together.

## Results

1. Table II shows that a significantly greater number of experimental than control Ss refused to change their opinions under opposition. The first prediction was thus confirmed. Since duration of all experimental debate sessions was about the same (around 10 minutes), it was not a factor in this result.

TABLE II

Shift of Opinion During Discussion

(Entries in this table are matched pairs of subjects. If entries are regrouped into a 2×2 table (shift vs. no shift), $p$ remains $<.01$.)

| | Experimental | | | Totals |
|---|---|---|---|---|
| | 0 | 1 | 2 | |
| Controls | | | | |
| 0 = no shift | 6 | 0 | 0 | 6 |
| 1 = qualified shift | 7 | 4 | 3 | 14 |
| 2 = shift to opposite opinion | 5 | 2 | 5 | 12 |
| Totals | 18 | 6 | 8 | 32 |
| Probability ($z = -2.83$) | | | | $<.01$ |

2. Table III shows that means of response latency for experimental and control Ss were about the same. This confirms the second prediction.

TABLE III

RESPONSE LATENCY
(i.e. time (in seconds) before *Ss* made their first statement)

| Subjects | Means | Variances |
|---|---|---|
| Experimental | 31.7 | 1,034 |
| Controls | 25.19 | 400 |
| Probability | | $< .01$ |

3. Table III shows also that the variance of the distribution for experimental Ss was significantly larger than for the control *Ss*. (N.B. This variability is due to inter-individual differences, notwithstanding any intra-individual differences which may also be present.)

4. Table IV shows a significant difference in median voice levels, experimental Ss speaking at a higher level than the control Ss. Range for all *Ss* is about the same, but is displaced along the scale for the two groups of *Ss*. Voice level correlates with the rating of ego-involvement, biserial $r = .44$. This suggests a relation between voice level and reaction potential.

TABLE IV

MEDIAN VOICE LEVELS (in decibels) AND RATED EGO-INVOLVEMENT
(number of *Ss*)

| | Medians | | | Range | Involvement rating | |
|---|---|---|---|---|---|---|
| | Total | Male | Female | | Yes | No |
| Experimental | .79 | .72 | 1.00 | −0.8 to +4.0 | 21 | 3 |
| Controls | −.04 | −.12 | .00 | −2.0 to +2.8 | 12 | 9 |
| Probability | $< .001$* | | | | .05 | |

*Sign Test.—Since each *S* had his own zero, the decibel scale is not a ratio scale and *t* cannot be computed legitimately.

5. Table IV shows also that the test of independence between involvement of Ss[6] in the situation and the score of emotional instability by $\chi^2$ gave: $p < .05$. This suggests that there is some relation

[6]Number of subjects rated for involvement is less than 32 owing to observers' omissions.

between involvement and initial reaction potential, and that both are related to voice level.

### Comment on the Design and Statistical Analysis

The primary purpose of investigation was to verify certain predicted relations between the variables involved. Generalization from sample to a particular population was of little importance, since we worked with two hypothetical populations: (*a*) the population of students from whom our sample was drawn, and (*b*) the universe of social situations from which our sample laboratory situation was drawn. Matching, which may represent a certain handicap in studies of the survey type, where generalization to a particular population is the primary goal, was considered an asset here, for it reduced the risk of Type II error by eliminating some variables which might be responsible for the results. There is evidence that our matching was relevant, because there was a correlation of .45 ($p < .01$) between the performance of the experimental and control Ss. Background variables correlated with performance were: course year, $r = .42$, $p < .05$; sex, $r = .28$, $p > .05$; income bracket of family provider, $r = -.24$, $p > .05$.

Because of the small size of the samples, no normality test was made. The distributions of latency were somewhat skewed, but the shape suggests that they could nevertheless have arisen from a normal parent. Deviation from normality, it has been shown, does not necessarily invalidate the $F$ and $t$ tests, but increases the risk of Type I error in using them. A sufficient guard against this type of error is obtained by raising the standard of rejection of the null hypothesis, for example from a confidence level of .05 to one of, say, .02. Our probabilities were obtained for latency dispersion by a $t$-test (21, p. 190), and for rigidity of opinion by a formula given to the writer personally by Dr. R. W. B. Jackson. As most $p$s were much above the confidence level mentioned, and in view of the large number of controls, confidence in the results appears justified. It should be remembered, however, that tests of significance were made with respect to hypothetical populations.

### Discussion

This study has shown that, in an opposition situation, emotionally unstable Ss tended to be more rigid in their opinions than the controls, that they tended to speak more loudly, and that their latency of response was more variable, anxious Ss being presumably slower, hysterics faster.

These relationships were predicted on the basis of deductions from a generalized behaviour theory. The confirmation of the hypotheses is

taken to mean that the conceptual system employed for derivation of hypotheses may be useful as a predictive device, provided that assumptions made can be verified experimentally, a task in which we are engaged at present. It is also convenient to think in terms of intervening variables and hypothetical constructs, because one can, if necessary, use various tests and measures without changing the conceptual system.

On the practical side, the present findings are an initial step toward assembling a series of results with which it may ultimately be possible to reverse the present procedure, and to predict certain personality characteristics, such as anxiety or neuroticism, from Ss' behaviour in controlled social situations. This would have obvious practical value in relation to assessment of mental health and in other fields of application.

The second paper in this series will report a study in which the situation was varied.

## Summary

Some 500 Arts students were screened for emotional stability by paper-and-pencil tests. Thirty-two high scorers were matched with 32 from the low end of the distribution on several variables. Experimental and control groups of three were formed, each with one subject and two role-playing assistants. They were compared with respect to the behaviour of the subjects as individuals facing verbal opposition from the other two members. This situation was constructed by using a system of generalized variables from the behaviour theory outlined in this paper, and predictions were derived from it deductively. Thus it was predicted that high-scoring, unstable subjects would show greater rigidity of opinion and greater dispersion of response latency. Both predictions were confirmed. Unstable subjects also tended to speak more loudly and were rated as more "involved" in the situation.

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# MANIPULABILITY OF BRAILLE CONTROL KNOBS[1,2]

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CONSIDERABLE experimental work has been done with differently shaped control knobs in an effort to reduce the incidence of confusion errors in the operation of controls. Most of the evidence presented in these studies is in terms of tactual discriminability rather than ease of manipulation or time and error involved in operation. The present study was undertaken to evaluate the comparative manipulability of eight differently shaped control knobs. Seven of these knobs are reproductions of those Jenkins (1) found most readily distinguishable by touch alone (both bare and gloved hand), in a group of 22 differently shaped knobs. Owing to a misinterpretation, the eighth knob, designated control knob "C," is a reversal of its counterpart in the Jenkins series.

Jones (2) ranked a number of control-knob shapes in terms of pilot preference. Four of the shapes ranking high on the preference list are included in the present set, and are designated as control knobs A, E, F, and H. Stump (3) found no significant differences in the manipulability of variously sized (¼", ½", 2") flat circular control knobs, thus indicating that size is not an important factor within the range tested.

## APPARATUS

The eight control knobs which are compared in the present experiment are shown in Figure 1. The knobs were constructed from wood, smoothly finished, and measured one and one-quarter inches in their greatest dimension.

The apparatus constructed for the experiment presented the subject with (*a*) a dial, three inches in diameter, at eye-level, and (*b*) an opening below and to the right of the dial, through which the subject operated the control knobs. The dial scale covered 300 degrees in a clockwise direction, with unit marks one degree apart and numbered in tens. Errors were read from a 10-inch duplicate dial with a synchronized pointer.

The control knobs were located about the circumference of a 14-inch vertical disc. A baffle plate behind each control knob allowed for manipulation by finger-tip only. By rotating the disc, the experimenter was

[1]Defence Research Medical Laboratories Report No. 165, Project No. D77-94-20-25 (H.R. No. 93).

[2]Braille control knobs are knobs distinguishable by touch alone.

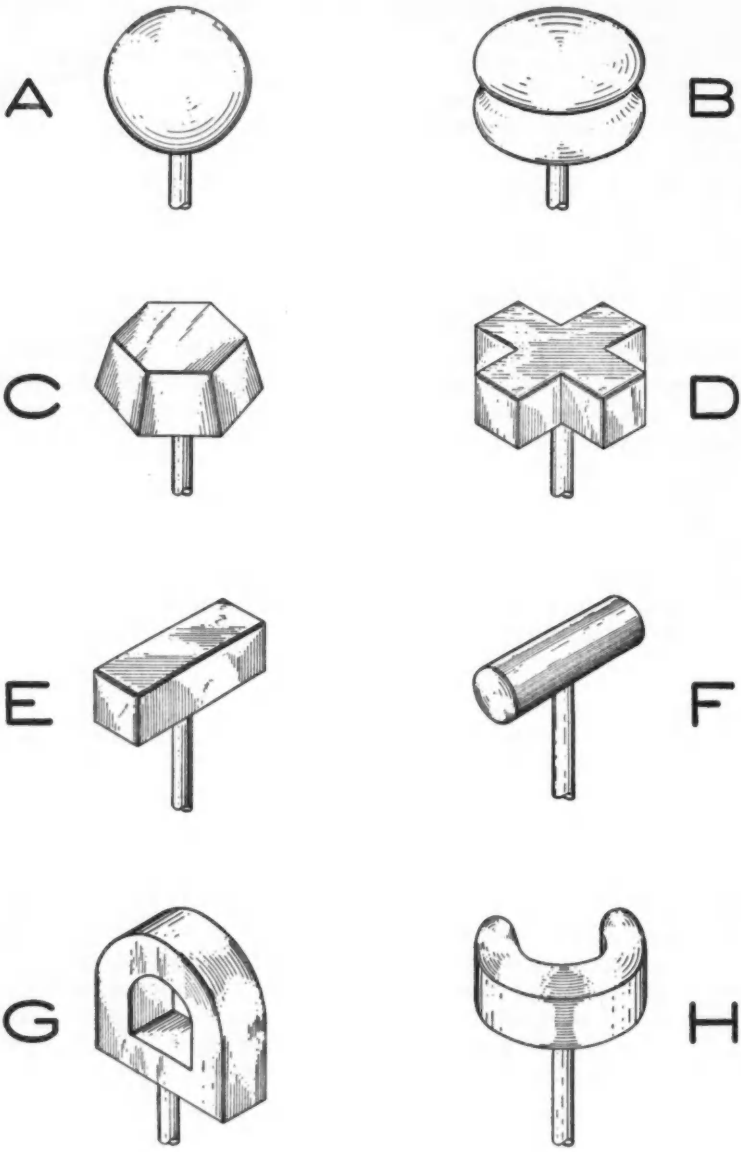


FIGURE 1

able to present the control knobs in any order. The control knob–pointer ratio was approximately 4:1, and they rotated in the same direction.

Between trials, the subject's hand rested on a platform below and in front of the control knob. A microswitch under the platform operated a timing device.

Subjects were required to make 64 settings in a single session. Eight settings were made with each of the eight control knobs. These involved pointer movements of 115°, 140°, 165°, and 230°, both to the right and to the left. The same eight settings were made with each control knob, and the trials were randomized for each subject.

### Procedure

The subjects were 72 aircrew flight cadets, ranging in age from 17 to 24.

Each subject was seated directly in front of the dial, the distance between the dial and subject's eye being governed by his personal preference. He was instructed to make all settings as quickly and as accurately as possible, and to avoid crossing the 30° scale break. The signal to proceed with a trial was "Set pointer at–." On the fourth trial the signal was shortened to "Set –," etc. This procedure was followed throughout the remainder of the 64 trials.

### Results

Data were recorded in terms of time and errors. Table I shows the mean time and error scores for the eight control knobs.

TABLE I

Mean Time and Error Scores (per setting) for the Eight Control Knobs

| Control knob | A | B | C | D | E | F | G | H |
|---|---|---|---|---|---|---|---|---|
| Mean error (in degrees) | 0.26 | 0.27 | 0.27 | 0.26 | 0.25 | 0.24 | 0.27 | 0.27 |
| Mean time (in seconds) | 3.98 | 4.26 | 4.61 | 4.37 | 4.39 | 4.33 | 4.26 | 4.57 |

Because the value of the mean errors is low, approximately one-quarter of a degree (equivalent to the width of the pointer tip), it would appear that an analysis of the errors would have no practical significance.

The results of an analysis of variance of the time data are presented in Table II.

TABLE II

ANALYSIS OF VARIANCE (time in seconds)

| Source of variation | Degrees of freedom | Sum of squares | Mean square | $F$ |
|---|---|---|---|---|
| Among knobs | 7 | 1230.99 | 175.86 | =3.19 |
| Within groups | 568 | 31309.80 | 55.12 | |
| Total | 575 | 32540.79 | | |

Analysis of these data ($F = 3.19$) indicates an overall variation in the speed of manipulation of the control knobs which is statistically significant at the 1 per cent level.

The confidence limits on the mean, at the 5 per cent level, show control knob "A" to be significantly superior to the remaining seven control knobs, with regard to manipulation time. Control knobs "C" and "H" are seen to be significantly inferior to the remaining six control knobs. Among the remaining five control knobs there are no appreciable differences.

## CONCLUSIONS

1. The basic conclusion to be drawn from this experiment is that statistically significant differences do exist in the time required to manipulate the control knobs which were investigated.

2. Further, the results suggest that in a control coding system using the present knob shapes it would be advisable to exclude those designated as "C" and "H" wherever the consideration of one-half second per setting is of practical importance.

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# CONGENITAL INSENSITIVITY TO PAIN AND ITS IMPLICATIONS FOR MOTIVATIONAL THEORY[1]

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PAIN is a central problem in medicine. It is also important to psychology, because it so effectively arouses a generalized emotional response. Unusual insensitivity to pain is of particular interest to psychologists, since pain has been given a key role in various theories of motivation, emotion, and personality development. This paper deals with that defect in response to noxious stimuli which can properly be labelled congenital universal insensitivity to pain, and with some of its theoretical implications.

## OTHER MANIFESTATIONS OF INSENSITIVITY TO PAIN

Congenital universal insensitivity to pain (hereafter CUIP) must be distinguished from absence of reaction under special conditions. For example, severe noxious stimulation may go unnoticed during extreme excitement, anger, or fear, as in combat or sports. Absence of reaction may also be found in the apathy of extreme depression. States of dissociation, like hysteria and hypnosis, show unusual indifference to noxious stimuli as a main characteristic. Reports of self-mutilation involving severe bodily damage are not uncommon in psychotic episodes (4). These conditions are distinguishable from CUIP on many grounds, an important one, in the examples given, being the relatively transient nature of the loss of pain reaction.

### *Mental Deficiency*

Mental defectives are frequently reported to show an insensitivity to pain which may appear like CUIP, but is accompanied by other developmental defects. Recent reports include a note by Keizer (17) on a 14-year-old mongolian idiot, and longer case studies by Arbuse, Cantor, and Barenberg (1), and Vega (31). In the Arbuse, Cantor, and Barenberg case (a 7½-year-old girl, IQ about 50) the absence of pain is a predominant feature, probably not entirely due to mental defect. In the other cases the indifference to pain is seemingly only part of a general dulling of response.

### *Brain Lesions*

Analgesia due to neural lesions has been much studied. In peripheral lesions, the circumscribed analgesia is clearly different from any general insensitivity. However, Schilder and Stengel (30) reported central lesions causing a more generalized lack of response to pain. They concluded that this was not a true analgesia, but a

[1]This article was prepared with the assistance of a summer research grant from the University of Saskatchewan.

cortical defect causing inability to evolve normal pain reaction; they called the condition "asymbolia for pain." Schilder and Stengel reported ten such cases, three of which, observed at autopsy, led them to believe that lesions in the left *gyrus supramarginalis* were the basis of pain asymbolia. Rubins and Friedman studied four patients showing pain asymbolia, which they define as "inability to recognize the unpleasant or disagreeable component of a painful or threatening stimulus, with the result that little or no defense reaction is produced, although the noxious stimulus itself is perceived" (29, p. 554). This resembles CUIP, except that in pain asymbolia there are other signs of cortical lesion. Rubins and Friedman's patients showed mixed aphasic symptoms, Gerstmann's syndrome, and other effects of left parietal lobe lesion. Similarly, six of Schilder and Stengel's ten cases showed typical sensory aphasia. Moffie (23) has also reported a case of asymbolia for pain. There was evidence of the insensitivity being congenital, but it was accompanied by other defects which the author attributed to arteriosclerotic lesions of the left parietal lobe.

The term "asymbolia for pain" has thus gained some acceptance as a description of unusual lack of reaction to pain in patients with lesions in the parietal lobe. Prefrontal lobotomy used for the treatment of intractable pain also seems to result in indifference to pain with no true analgesia. Fulton (11) believes that this relief is produced because the lobotomy severs fibres projecting from the reticular formation of the brain stem to various parts of the forebrain, including the limbic cortex. It is of interest that indifference to pain, without other marked deficits, may follow an operation which destroys relatively small areas in the medial ventral quadrant of the frontal lobe (7). This suggests the possibility that CUIP may be due to similar neural defects, with the difference that their origin must lie in some developmental anomaly rather than in surgical intervention or disease process.

## Congenital Universal Insensitivity to Pain

### *Criteria*

The criteria for true CUIP are largely contained in the name.

1. There is usually fair evidence that the condition dates from birth, or has at least been observed at a very early age and over a period of years.

2. The insensitivity to pain is universal, i.e. to all types of pain throughout the body. This is hard to show conclusively, as noxious stimuli of high intensity are difficult to administer. There are also enough exceptions, where some pain is reported (10, 16, 18, 19, 33), to suggest that this is probably not an all-or-none phenomenon. Nevertheless, the insensitivity should have sufficient generality to be clearly different from any localized loss of pain.

3. We are concerned with "insensitivity" rather than "indifference" to pain. Many authors have preferred the latter word, since these subjects are not unaware of pain-producing stimuli, but simply fail to show an adequate reaction. A pinprick, for example, is easily recognized. The sharp prickly quality of heat stimulation is also perceived,

and enabled McMurray (22) to establish in the patient a threshold close to the normal heat pain threshold. The recognition of noxious stimuli may, however, be due to learning based on accompanying sensory stimulus patterns. In any case, the many accidents suffered by these patients could hardly have occurred through mere indifference to pain. During the investigation of Case 6 (Table I), for example, burns were accidentally produced while using the Hardy-Wolff-Goodell pain threshold apparatus (13). This could not have resulted from indifference to pain, because the subject was certainly not indifferent to tissue damage, and would have avoided it if she had suspected its occurrence.

4. Absence of reaction is relatively specific to pain, though other mild sensory defects may be found. Three case reports (18, 19, 22) noted moderately impaired temperature sensibility; two (2, 16), mild defects in the sense of smell. In a recent case, Kipnis and his associates (18)

TABLE I

MAIN SERIES OF CASES OF CONGENITAL UNIVERSAL INSENSITIVITY TO PAIN

| Subject | Author | Date | Age | Sex | Data regarding general functioning level |
|---|---|---|---|---|---|
| 1 | Ford & Wilkins (10) | 1938 | 9 | M | IQ 104, mild congenital word blindness |
| 2 | Ford & Wilkins (10) | 1938 | 8½ | M | IQ 76, epileptic convulsions at age 2 |
| 3 | Ford & Wilkins (10) | 1938 | 7 | F | IQ 86 |
| 4 | Kunkle & Chapman (19) | 1943 | 25 | M | Average intelligence, epileptic |
| 5 | Boyd & Nie (2) | 1949 | 7 | F | IQ 92 |
| 6 | McMurray (22) | 1950 | 22 | F | IQ 128 |
| 7 | Farquhar & Sutton (8) | 1951 | 7 | F | IQ 72 |
| 8 | Jewesbury (16) | 1951 | 34 | M | Raven's Progressive Matrices—above average |
| 9 | Jewesbury (16) | 1951 | 38 | M | Raven's Progressive Matrices—average |
| 10 | Jewesbury (16) | 1951 | 76 | M | Good intelligence |
| 11 | Jewesbury (16) | 1951 | 3½ | M | Average or better development |
| 12 | Von Nissler & Parnitzke (33) | 1951 | 9 | M | Average intellect, good school marks |
| 13 | Westlake (34) | 1952 | 6 | F | Average intelligence |
| 14 | Von Cerny-Waldvogel (32) | 1952 | 2 | M | Average intelligence |
| 15 | Girard, Devic & Garin (12) | 1953 | 1½ | F | Early development normal |
| 16 | Kipnis, Cohen, Kubzansky, & Kunkle (18) | 1954 | 19 | F | Very superior intelligence level |

noted slight impairment in touch, two-point discrimination, texture recognition, barognosis, and graphaesthesia. However, the facts that no major sensory defects other than pain are reported, and that many cases show normal or accelerated intellectual development, leave little doubt that CUIP is a relatively isolated phenomenon.

5. Finally, there is an important negative criterion; namely, that the insensitivity to pain is not attributable to syringomyelia, any extensive central nervous lesions, neurosis, psychosis, or mental deficiency.

Table I shows the main series of cases which appear to satisfy the above criteria.[2]

*Clinical Description*

The clinical features are remarkably consistent. There is usually a series of incidents revealing unusual insensitivity. These include cuts, bruises, burns, tooth extractions, broken bones, and various painful illnesses. Case 1 broke his left fibula at the age of two, but continued to walk. When eight years old, he sat on a hot steam radiator until severely burned. Case 6 also suffered third degree burns from kneeling on a hot radiator to look out the window. Many of these patients have bitten their tongues severely. Cases 1, 5, 6, 12, 14, 15 all showed permanent deformation of the tongue, having bitten off the tip in early childhood.

Eventually, these cases came under observation either because of the defective pain response or because of some illness or injury. The resultant reports establish clearly the five main criteria of CUIP as outlined above, each feature standing out sharply, in spite of the relatively small number of cases.

*Pain Tests*

The examination of pain reaction was done in many ways. All the usual noxious stimuli—pricking with a pin, electric shocks, pressure on sensitive parts, hair-pulling, burning, and pinching—were used. Other tests included the Hardy-Wolff-Goodell pain threshold apparatus, the Lewis (20) test for the production of muscle pain, the production of "cold pain" from immersion of the hand in ice and water, oesophageal distention, and the production of headache by injection of histamine phosphate.

Absence of reaction in nearly all these tests means that the patients

[2]Madonick recently reported the case of a 38-year-old Negress. Although mentally retarded, she showed typical CUIP, and the author believed that the mental retardation did not affect the results. It is interesting to note that the cold pressor and other physiological responses to pain were absent. (Madonick, M. J. Insensitiveness to pain. *Neurology*, 1954, 4, 554-557.)

did not show the usual behaviour of withdrawal, attack, wincing, crying out, or reporting pain. These are, of course, clinical observations in which the clinician judges the absence of reaction. This fact, however, should not discredit the reports, since the normal response is so obvious that the marked difference shown by these patients needs no precise measurement. Also, the most striking evidence of insensitivity to pain rests usually in the patient's past history of indifference to everyday noxious stimuli, many of them much more damaging than could be administered for purposes of examination.

Nevertheless, it is unfortunate that so few of these investigations included observations of blood pressure, heart rate, respiratory, psychogalvanic, and other physiological changes which frequently accompany pain. The few who have used such measurements show some disagreement. Ford and Wilkins (**10**) reported no psychogalvanic reaction in response to deep pinprick, and no change in pulse as a result of pressure on the Achilles' tendon. Kunkle and Chapman (**19**) used a blood pressure and pulse measure in the "cold pain" test; the patient did not report pain or show overt reaction, but both systolic and diastolic blood pressure rose, and heart rate increased. Jewesbury (**16**) mentions in Case 8 that the patient did not object to strong faradic shock, and showed no increase in systolic blood pressure. McMurray (**22**) used continuous recordings of systolic blood pressure, heart rate, and respiration, during immersion of a hand in water at 0°-2° C., and, in a second test, at 49°-51° C. The records showed an absence of change which differentiated the patient's reaction completely from that of control subjects. Systolic blood pressure was recorded in another experiment using electric shock as the stimulus. Again the patient's systolic blood pressure rose only 1.3 mm., while eight control subjects showed a mean rise of 19.1 mm. Kipnis (**18**) found the usual pressor response to repeated contractions of muscle in an ischemic limb, antidiuresis to intense cold or muscle pain, and alpha-wave inhibition by skin injury all absent in their subject. The cold pressor response, however, remained intact.

If such data were available for all these cases it would be possible to say whether there is, in these patients, a consistent lack of change in autonomic functions corresponding to the lack of behavioural reaction. In spite of certain discrepancies, the indication is that there may be. If so, this striking observation would aid greatly in the definition of CUIP.

*Defects Associated with CUIP*

Reports of this pain defect contain many incidental observations, some perhaps of major importance. For example, in Cases 4, 6, 16, the

patients had never experienced itch. This appears to support the hypothesis that itch is the response to a pattern of stimulation, prominent in which is the near threshold activity of pain receptors. Hardy, Wolff, and Goodell (13, p. 216) cite Cases 4 and 6 in support of this idea. The main difficulty with this conception is the practical certainty that peripheral pain receptors and fibres are normal in CUIP. Feindel's (9) study of Case 6 confirmed earlier observations made in skin biopsy, which were reported briefly by McMurray (22). Cases 15 and 16 also showed apparently normal innervation in skin biopsies. Some central elaboration of the itch pattern may be defective in these patients, as it is for more obviously pain-producing stimuli.

A striking number of patients have had some bone or joint disease: Case 1 showed atrophy of the scaphoid bone; Case 5 chronic osteomyelitis of the left leg and ankle; Case 6, osteomyelitis and arthropathy reported in detail by Petrie (26); Case 12, a congenital luxation of a hip joint with other bone and joint defects; Case 13, arthritis; and Case 14, an osteochondroma in the left foot. Petrie (26) suggests that some of these conditions may be directly due to lack of the protection given by pain sensation. Feindel (9), discussing the role of pain sensibility in the joints, also concludes that absence of pain can lead to progressive joint changes resulting in arthropathy. A similar opinion is expressed by Von Nissler (33). The remarkable number of broken bones in this series also suggests absence of normal precaution in work and play.

*Other Reports of CUIP*

In addition to the fairly complete reports in Table I, many brief, anecdotal reports of other cases have appeared. Dearborn (6) in 1932 described a typical case, and Critchley (5) in 1934 mentioned two others. Grain and von Hagen, in discussing Boyd and Nie's (2) paper, reported briefly on three patients. Leys (27) published a note about a two-year-old defective girl who, he believed, showed congenital insensitivity to pain. Keizer (17) in a letter mentioned three cases; one an idiot, who would be ruled out of this series, the others two women in whose cases, it was reported, painless parturition led to accidental death of the newborn. These reports are of interest, but add little information to the main series.

More recently Rose (28) described a three-year-old boy admitted to hospital for treatment of osteochondritis of the talus. The boy's insensitivity to pain was discovered, and questioning of the mother indicated that it was congenital. Later there appeared to be a gradual return of normal pain reaction. This and Jewesbury's (16) report of Case 11 are the only examples noted of such a return of sensitivity.

## Psychological Implications

### *Relation to Anxiety*

According to many theories, complete, lifelong absence of reaction to pain should markedly affect the emotional and motivational structure of the individual. Mowrer, for example, asserts that "*anxiety (fear) is the conditioned form of the pain reaction*, which has the highly useful function of motivating and reinforcing behavior that tends to avoid or prevent the recurrence of the pain-producing (unconditioned) stimulus" (**24**, p. 17). Starting from this assumption, he makes anxiety reduction an all-important source of secondary reinforcement, and a key to development of the complex motivational structure of modern man. In a footnote to the above quotation, Mowrer somewhat qualifies his statement by suggesting that there may be both conditioned and unconditioned fears, the unconditioned form occurring as an innate reaction to noxious stimulation, parallel to the pain reaction.

From either position, however, it would seem that CUIP should result in defective personality development. This, however, does not appear to be the case. Certainly, lack of normal anxiety has not been generally observed. It seems unlikely that such an important personality characteristic would completely escape the clinical interview and intelligence tests, and be missed, in two studies at least (**18**, **22**), in projective tests and personality inventories.

### *Derived Drives*

Mowrer's position is typical of that held by a large group. Brown (**3**), for example, attempts to account for motives such as those for money, prestige, and affection in terms of the single principle that stimuli associated with the lack of any of these arouse anxiety, and that responses which reduce this anxiety become powerfully reinforced. He summarizes the acquisition of a drive for money as follows: (1) cues supplied by the behaviour of worried parents, whenever a child experiences pain, come to elicit anxiety reactions in the child; (2) similar parental behaviour associated with cues signifying lack of money results in these cues also eliciting anxiety.

Thus upon the primary drive of pain there is erected a considerable superstructure to account for adult motives. But the evidence is growing that an individual may have a lifelong absence of pain, without other marked defects, and may yet develop the motives characteristic of our culture. However attractively and precisely the theory is advanced that complex human motivation is derived entirely from biological drives, fundamental objections continue to appear. Mowrer was aware of the objections raised by these cases of CUIP, and specifically cited Case 6

as an example of the importance of pain for survival and adaptation (25). He did not deal with the objections directly, because he regarded this case as very exceptional. It is probable, however, that in the absence of pain he would attribute the eliciting of primal fear response to suffocation, loss of support, and other forms of intense stimulation. It is unfortunate that more exhaustive personality studies of these patients are not available, so that the anxiety manifestations could be more closely checked, as well as the possibility of these other primary drives substituting for pain in the learning process. Detailed investigation is necessary if we are to uncover any common characteristics of personality, or even learn how to look for the consequences of this radical change from normal response in such a wide and important category as pain.

*Other Viewpoints*

Many investigators, like Hebb (15), Harlow (14), and McClelland (21), argue that motivation must be built on a broader base than is provided by derived-drive theories. According to Hebb, "the term motivation then refers (1) to the existence of an organized phase sequence, (2) to its persistence in a given direction, or stability of content" (15, p. 181). From this viewpoint pain, as a disruptive process, could serve as a channeller of motivation by affecting the pattern of the phase sequences. Pain, or the threat of pain, would normally be one of the main means of everyday control. It is not indispensable, however, since from Hebb's standpoint many other events could serve to channel and limit the variety of conceptual activity, and hence be motivating.

Harlow (14) has emphasized the role of external stimuli in motivation, insisting that they elicit motivated behaviour patterns not based on any internal tension state or need. The absence of primary pain drive would, to him, have no serious effect on the formation of other motives, or on normal learning.

McClelland bases motivation on affect, and postulates that "positive affect is the result of smaller discrepancies of a sensory or perceptual event from the adaptation level of the organism; negative affect is the result of larger discrepancies" (21, p. 43). Cues associated with changes in such affect acquire power to redintegrate affective states and hence are motives. According to this theory, the affective reaction to discrepancies between perception and expectation is innate, but the association of this reaction with relevant cues is learned; in this sense "all motives are learned" (21, p. 28). McClelland also postulates that "affective arousal is the innate consequence of certain sensory or perceptual events" (21, p. 43). Pain would normally be an important one of these, but its position is not unique, and the main basis for affective

arousal remains the reaction to discrepancies between adaptation level (expectations) and perception. None of these views encounters the difficulties of the derived-drive theories in explaining the apparently normal motivation and learning of individuals with CUIP.

## Interpretation of CUIP

There have been no satisfactory explanations of CUIP. Most authors regard it as a congenital developmental defect which has somehow selectively affected neural mechanisms concerned with pain reaction. Jewesbury (16) points out several features suggestive of cerebral maldevelopment: mild congenital word blindness in Case 1; some evidence of epileptic attacks in Cases 2, 4, 8; low general intelligence in Cases 2, 3, 7; and a tendency for left-handedness (mixed dominance) in Cases 5, 8, 9, 10. There is, however, little or no such evidence of cerebral malfunction in Cases 6, 12, 13, 14, 15, 16. In any event, the explanation remains unsatisfactory until more can be done to specify what sort of developmental defect has occurred, where it can be found, and how it arises.

## Comment

Little progress can be made in the explanation of CUIP until further advances are made in the understanding of pain as a neural pattern. Although reports have increased in recent years, the defect is still comparatively rare, and investigation has been limited to certain clinical approaches. Coordinated effort is required in three main directions: 1. extension of the investigation of pain response to include (*a*) a full series of pain tests, not merely such simple procedures as pricking with a pin, pinching, or pulling hairs, and (*b*) measurement of the integrated physiological reactions of the body to noxious stimulation; 2. clearer establishment of the degree to which CUIP is an isolated phenomenon, through more exact threshold measurements and more careful search for clinical signs of neural malfunction; 3. better understanding of the role of pain in learning, motivation, and emotion through more systematic psychological studies of CUIP cases, e.g., long-term studies on cases discovered at an early age, and more complete clinical investigation at all age levels.

## Summary

All known cases of congenital universal insensitivity to pain were considered in an attempt to establish the criteria by which this defect

may be recognized. It was concluded that this is a congenital defect causing a general insensitivity to pain, with relatively little involvement of other modalities, and with no general mental or physical retardation. The implications for theories of the formation of human motives were discussed.

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## BOOK REVIEW

*The Management of Mental Deficiency in Children.* By I. NEWTON KUGELMASS. New York: Grune and Stratton, Inc., 1954. Pp. xii, 312. $9.50.

WHILE, in the past, major professional texts on mental deficiency were designed exclusively for the medical, psychological, or educational worker, the present book is not designed for a specific group. The author has recognized that his subject matter does not support professional disembodiment and has accordingly chosen an inter-disciplinary approach towards mental deficiency.

Former textbooks tended to be organized around etiological concepts, as if such concepts were more than transitory and highly capricious "best guesses" about causation. The present book explores mental deficiency from the viewpoint of symptomatology, rather than etiology, and thereby achieves a theoretically less precise but more realistic basis for classification. From such realism stem useful suggestions regarding both management and treatment. In sacrificing the traditional etiological orientation the author has blunted the negative view of treatment taken by earlier writers, subject as they were to the vagaries of etiological speculation.

The clinical psychologist will find the psychological aspects of this book neither highly sophisticated nor penetrating. He will, however, appreciate the novel emphasis placed on community and non-institutional problems of the defective child; on treatment and training at lower intellectual levels; and on the more than adequate medical coverage of symptomatology and the "rare" syndrome. Perhaps, as in the past, an over-emphasis is placed on the rare clinical syndrome and too little on the less dramatic but vastly more numerous instances of high grade defectives of the so-called endogenous etiology.

In the main, this publication is slanted towards the medical profession but at a level well within the technical grasp of others.

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